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ORIGINAL LECTURES.

CARTWRIGHT LECTURES.

ON CERTAIN PROBLEMS IN THE PHYSIOLOGY OF THE BLOOD CORPUSCLES.

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Physicians and Surgeons, New York, March 30, 1886.*

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See p. 365. LECTURE III.

THE RELATION OF THE CORPUSCLES TO COAGULATION AND THROMBOSIS.

I PROPOSE, in this lecture, to consider the question of the relation of the corpuscles to the processes of coagulation and thrombosis, and I will first call your attention to the action of the *colorless corpuscles*. Our knowledge of the connection between these elements and coagulation dates from the observations of Buchanan in 1831. He attributed the action of what he called washed blood-clot, in inducing clotting, to the colorless blood corpuscles included in the meshes, and which he said acted as a sort of ferment, comparing the action to that of rennet. These views have been greatly elaborated by Schmidt, of Dorpat, and his pupils, to whose researches we are indebted for an important extension of our knowledge in this department of physiology.

According to these well-known observations, the colorless corpuscles furnish the fibrinoplastin or paraglobulin, and the ferment, while the third element, the fibrinogen, exists naturally in the blood plasma. Schmidt and his pupils hold that, in furnishing these two elements to make up the fibrin, the colorless blood corpuscles undergo disintegration and destruction. Part of the evidence which they bring forward in proof this is as follows: The blood plasma of the horse may be readily collected by keeping the blood at a low temperature and allowing the red blood corpuscles to subside, when a clear layer remains, consisting of plasma with a few red and many colorless corpuscles. Now, if a portion of this plasma is taken and whipped with twigs, the difference between the number of colorless corpuscles remaining in the serum and those in the original plasma represents the number of colorless corpuscles which have undergone destruction in the process of the formation of fibrin, and Schmidt and his pupils estimate that at least seventy per cent. of the colorless corpuscles undergo destruction in this way. They found that, instead of 15,000 colorless corpuscles in a cubic millimetre of the plasma before it is whipped—i. e., before the fibrin is extracted—there were subsequently not more than 4000 per cubic millimetre remaining in the serum. Examining the clot so obtained, it is stated that the colorless corpuscles have largely, if not entirely, undergone destruction in the formation of fibrinoplastin and the fibrin ferment. This is, perhaps,

the most convincing experiment which any one of Schmidt's pupils has brought forward to sustain the view, that colorless corpuscles undergo destruction in the process of coagulation. There are many other points urged by Schmidt to which I need not refer, as they are readily accessible in the works on physiology.

The researches of Wooldridge¹ have also shown that the colorless corpuscles play an important part in the formation of fibrin. He has been able to procure leucocytes from lymph glands in a tolerably pure condition, by means which he has described at length in his paper. These leucocytes when added to an equal volume of a ten per cent. solution of common salt seem to be converted into a material resembling very closely ordinary fibrin. By experimenting with what is known as peptone plasma he has obtained very striking results which would appear to indicate still more clearly that leucocytes play an important part in this process. Peptone plasma is obtained by injecting peptone into the bloodvessels and then bleeding the animal. Coagulation is prevented entirely by the influence of peptone, and the red blood corpuscles may be entirely removed from the serum by the centrifugal machine. This plasma shows no special inclination to coagulate, and is, of course, particularly suitable for experimental purposes. If the leucocytes prepared from the lymph glands be added to this plasma, coagulation at once occurs. If a small quantity of leucocytes is added, the amount of fibrin produced is small; if a larger quantity is added, more fibrin is produced. In fact, Wooldridge has shown that the amount of fibrin produced in the peptone plasma is directly proportionate to the leucocytes added. The leucocytes seem themselves to form the fibrin—perhaps the entire mass, for the weight of the fibrin produced is the same as the weight of the leucocytes added. Moreover the albumins in the peptone plasma, after coagulation, can be shown not to have undergone any change, but remain the same, quantitatively and qualitatively; and a third point is that the leucocytes appear to have undergone disintegration.

There are other points in Wooldridge's researches to which I shall not have time to refer at length, but he concludes that it is only the dead plasma which converts the cells into fibrin, as the injection of leucocytes into the blood of the living dog produces no effect.

Such facts appear to show very conclusively that the corpuscles do undergo disintegration, and yet if the blood plasma of the horse is examined after it has been whipped leucocytes may be found in the serum and also in the clot which has been produced, so that all the leucocytes have not undergone destruction. The existence of a certain number of the leucocytes after clotting has occurred, has caused one of Schmidt's pupils, Heyl,² to divide the leucocytes into two sets: the alpha-leuco-

¹ Proceedings of the Royal Society of London, 1881.

² Dorpat Dissertation, Fortschritte der Medicin, 1883.

cytes, which undergo destruction during clotting; and the beta-leucocytes, which remain. From observation, I do not believe that the number of the leucocytes which undergo disintegration in the clotting of the horse's blood is anything like so extensive as Heyl states.

Although the evidence in favor of the destruction of the colorless elements seems conclusive, yet, if the fibrin formation is studied under the microscope, it appears to take place without any disintegration of colorless corpuscles, and it is extremely difficult to demonstrate their participation in the process. As is well known, it can be studied in a blood-drop examined in the ordinary way, or, better still, in the moist chamber. The time which elapses before coagulation begins is variable in different individuals and under different conditions. Usually, however, from fifteen seconds to two or three minutes elapse before the first appearance of the fibrin filaments is noticed. A slide can be prepared in a very few seconds, and there is sufficient time before clotting begins to examine the colorless corpuscles, the red corpuscles, and the blood plaques. I must say that, in a very careful examination of the process of the formation of fibrin in this way, I have never seen any appearance in the leucocytes which would indicate that, as the fibrin was formed, they underwent disintegration or dissolution. On the contrary, they seem most stable elements, and the ameboid movements persist long after the fibrin network is thick and dense in the field. Certainly in the microscopical examination of the ordinary slide, or in the examination of the blood-drop in a moist chamber, I do not think anyone has seen the direct disintegration of leucocytes in the production of fibrin. An interesting and instructive experiment is to draw the blood of a frog, or of the horse (in which Schmidt and his pupils hold that the colorless corpuscles so rapidly undergo disintegration), into a fine capillary tube in which the process of clotting can be watched under the microscope. At first, the entire tube is filled with corpuscles; but, before long, it is seen that the clot contracts, and there is a peripheral layer of serum squeezed out. In a short time, leucocytes can be seen emerging from the clot in numbers, either squeezed out or migrating from it. This experiment, which can be readily demonstrated, forms an admirable mode, as Schäfer showed some years ago, of studying the process of coagulation.

A study of the histogenesis of fibrin as seen in the moist chamber, in the capillary tube, and on the ordinary slide, affords, I think, no evidence in favor of the destruction of the colorless corpuscles, but, on the contrary, is directly opposed to this view. In a certain number of instances the aggregations of blood plaques, to the connection of which with the process of coagulation I shall shortly refer, have possibly been mistaken for colorless corpuscles.

The relation of the *red corpuscles* to coagulation is not regarded as very important; they play a more passive part. But Landois and others have described a process which can be readily seen in the blood of the frog and in mammalian blood, examined in serum. If we take the blood of the frog and examine it in the serum of the blood of the rabbit, it will be seen that the red corpuscles of the frog crowd into columns, and in a short time the hæmoglobin leaves the corpuscles, which become granular, and fibrin filaments form

in their vicinity, and, according to Landois, the red corpuscles break down into a material which resembles granular fibrin very closely, indeed. These observations were made ten or eleven years ago by Landois, and they have been confirmed by others; but whether the corpuscles undergo transformation into the fibrin filaments, or whether fibrin only clots about these groups of corpuscles under the influence, perhaps, of a ferment which they extrude, it is impossible to say.

The relation of the *blood plaques* to coagulation is particularly interesting, and is, at present, attracting a great deal of attention.

In the study of fibrin formation, as seen under the microscope, it has long been noticed that the fibrin filaments spread out as distinct rays from the minute aggregations which have been known as Schultze's granular masses. Schultze noticed these, as did also Ranvier, in 1873, who regarded these masses as centres of coagulation. That the fibrin sets in a thick, dense network about the plaques is readily seen, but it can also be noticed, particularly if healthy blood is examined in which the plaques are not very numerous, that the fibrin also appears quite independently of the plaques. It forms as distinct little needle-shaped bodies presenting an appearance not unlike that of crystals. That these crystal-like portions of fibrin appear in regions of the field quite apart from the blood plaques, is well seen in studying the process of coagulation in the moist chamber. Although the fibrin needles when first formed may appear in portions of the field unoccupied by blood plaques, yet the network is usually most dense in their neighborhood, and when the entire field is covered with fibrin filaments, the disintegrated blood plaques look like centres from which the filaments radiate.

The relation of the blood plaques to coagulation, as examined experimentally, is even more interesting. If an ordinary ligature is passed through the femoral vein of a dog and allowed to remain for five or six minutes, or even less, the threads become coated with the plaques, as represented in Fig. 1. It is well to separate

FIG. 1.



Aggregation of plaques on a thread of cotton passed through femoral vein of dog and allowed to remain ten minutes.

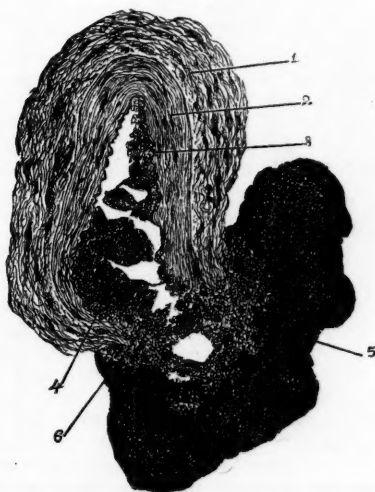
slightly the filaments of the thread, and if exposed to the blood stream for as long as ten minutes they become uniformly beset with the plaques. A few white corpuscles may be entangled among them, but undoubtedly the plaques are the first elements to aggregate about such a foreign body. The outlines are usually distinct, but if allowed to remain long in the vein those nearer the threads become more granular, and the distinct corpuscular nature is less evident. If a small brush of thread is tied to the end of pen-handle, or any suitable object, then whipped in the blood, as freshly drawn, for four or five minutes, and then examined, the brush of threads will have precisely the same appearance, and are uniformly covered with blood plaques. The colorless corpuscles are adherent here and there, but the blood plaques form the striking elements. They adhere to the filaments of the thread, and several of the finer

fibres of the thread may be entirely agglutinated by the aggregation of the blood plaques about them. I can fully confirm these original observations of Bizzozero and Hayem, and the experiments have now been repeated by a number of observers. If the threads, after having been whipped in the blood, are carefully washed in a saline solution, all the red corpuscles can be washed away, so that few, if any, can be seen, and then if these threads are dipped into a coagulable solution, clotting will occur. This experiment was performed by Bizzozero in 1882, and has been repeated by other observers. It has been urged against it that possibly the threads beating about in the blood have absorbed some of the fibrin ferment. This is, of course, possible, but certainly in such threads the chief elements to be seen are the blood plaques, and the leucocytes are very scanty; besides, the greater the number of the blood plaques adherent to the thread the denser the coagulum will be, as if the blood plaques furnished the material for the production of the fibrin or the ferment in large quantity.

Still more conclusive evidence of the participation of the blood plaques is their relation to thrombi, as experimentally produced.

The femoral artery of a dog is exposed and a linear slit made in the vessel, through which the animal is allowed to bleed to death. This portion of the vessel is rapidly excised and placed at once in alcohol, or, still better, first in osmic acid, and then sections carefully cut through the part where the incision was made, when such an appearance as seen in Fig. 2 will be found. Occupying

FIG. 2.



Section of femoral artery of dog at the site of longitudinal incision through which the animal bled to death. (Cut rather obliquely, low power.) 1, 2, 3. Adventitia, media, and elastic lamina of intima. 4. Aggregations of blood plaques in enormous numbers about the intima and the cut margins of the vessel. 5. Clot composed chiefly of red corpuscles. 6 X. The cut end from which Fig. 3 was sketched.

the cut edges, and filling in places the lumen of the vessel a finely granular material is seen under a low power. Surrounding it, to the outside, as represented at 5 in the

figure, there is a darker material made up largely of dark clots composed of red blood corpuscles. In the central portion, in immediate contact with the cut edges of the vessel, in contact with the elastic lamina of the intima, and occupying the interstices of the ragged surfaces, are the blood plaques. This was so stated by Bizzozero in 1882, and it has been confirmed in an elaborate investigation from the laboratory of Langhans,¹ in Berne. My own observations are in harmony with these, and we may say that the plaques are the elements which first settle on the edges of a wounded vessel and which form the basis of the thrombus.

Fig. 3 represents the end of a portion of the adventitia indicated by a cross (X) in Fig. 2. The sketch

FIG. 3.



End of small portion of adventitia indicated at X in Fig. 2. The fibres are everywhere surrounded with granular disintegrating plaques.

shows the blood plaques in a condition of granular disintegration, but under a high power the outlines can be distinctly defined, and any one with a knowledge of these elements and of the changes they undergo has no difficulty in recognizing them. If the cut ends of the vessel are examined when fresh, in osmic acid or Pacini's fluid, the elements are still more clearly seen and are readily determined to be identical with those in the circulating blood and in the granule masses. The elaborate investigations of Eberth, published in the January number of Virchow's *Archives*, 1886, clearly demonstrate that the plaques are the first elements to settle and lodge on the lacerated portion of the vessel or on a portion of vessel destroyed by acid or by caustic.

The relation which the blood plaques bear to the so-called white thrombi is particularly interesting. Zahn² appeared to prove by his observations that white thrombi are composed exclusively of colorless corpuscles, and the current idea is that to a lacerated portion of a vessel the colorless corpuscles adhere and undergo disintegration, become granular, and form in this way a white

FIG. 4.



Plaques from thin clot on warty endocarditis.

thrombus. Bizzozero, Hayem, and Eberth have shown, I think pretty conclusively, that if a needle is passed across a vessel in the omentum or in the mesentery, so as to injure it, the first elements which are collected at the site of the injury are not the colorless corpuscles, or the red corpuscles, but the blood plaques, which form

¹ Lubnitzky, Archiv, f. Exp. Path. u. Pharm., 1883.

² Virchow's Archiv, Bd. 62.

distinctly aggregated masses—white thrombi. There may be colorless corpuscles as well, but the chief bulk of the thrombus, which has formed at the site of the injury, is undoubtedly made up of blood plaques.

A study of white thrombi as met with in man leads us to the same conclusion. These structures have been long recognized, and have been supposed to be made up largely of colorless corpuscles. We find them on atheromatous ulcers, forming thrombi in the femoral veins, in the auricles and ventricles, on the valves in endocarditis and as the lining of aneurismal sacs. The examination of the superficial part of a white thrombus in osmic acid, Pacini's fluid, or even salt solution, reveals the fact that it is composed of blood plaques. In the peripheral part where they have not undergone disintegration, such thrombi are, so far as my observation goes, without exception, made up of small circular, disk-like elements which any one familiar with the blood plaques will readily recognize as such. Fig. 5 represents

FIG. 5.



White thrombi composed almost entirely of blood plaques. Abdominal aorta. Woman dead of cancer of the stomach. From specimen in Museum of McGill Medical Faculty, Montreal.

two or three white thrombi in the aorta immediately above the bifurcation. The case was one of cancer of the stomach, and when the aorta was slit open these masses were seen looking as if a neoplasm from the retroperitoneal glands had perforated it. They were grayish-white in color, soft, and on examination were seen to be composed of the elements shown at Fig. 6. There

FIG. 6.



Plaques from specimens illustrated in Fig. 5.

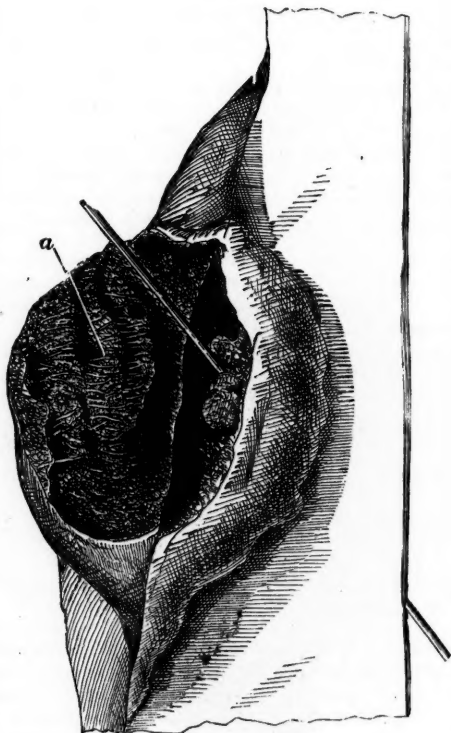
could be no doubt as to their nature; they were blood plaques, presenting the circular appearance, and on profile, the narrow linear aspect of these bodies. This was the first specimen in which I was able to demonstrate that the white thrombi were made up of the blood

plaques. Since then many specimens have fallen under my observation, particularly in connection with vegetations on the valves of the heart, the thrombi in aneurisms, and upon atheromatous ulcers. I would ask those specially interested in the question carefully to observe the white thrombi, more particularly the superficial parts of them in contact with the blood current. I think they will find that, without exception, they are composed not of colorless corpuscles, nor of a reticulated fibrin network, but almost exclusively of these plaques which, in the deeper parts, have undergone granular disintegration, but in the superficial parts still retain their normal shape and appearance.

The observation that these white thrombi consisted of blood plaques was confirmed in 1882, by Bizzozero, and, in the same year by Hayem, and since then it has been noted by a number of observers.

Fig. 7 represents a small aneurism of the thoracic aorta, which shows on its lining membrane a number of

FIG. 7.



Small aneurism of thoracic aorta, showing the internal wall of the sac covered with numerous curvilinear elevations, grayish-white in color, and composed of blood plaques. Specimen in Museum of McGill Medical Faculty, Montreal.

soft grayish-white curvilinear elevations, such as all observers have noted. On examination these will be found to be made up of elements similar to those which compose the white thrombi, namely, distinctly circular, disk-like blood plaques. The changes which these bodies undergo are very peculiar. As I mentioned in my first lecture, they appear in masses as soon as they

are withdrawn, and then undergo remarkable transformation, whereby they lose their outline and become converted into a granular material in which the individual plaques become unrecognizable. That change occurs in the blood plaques as they form these white thrombi. In the deeper portion of the thrombus, represented in Fig. 5, the blood plaques had disintegrated and become granular and were no longer distinctly recognizable; but at the superficial part they were distinct, their outlines were well marked, and in osmic acid, in teased preparations, distinctly made out and readily preserved.

Eberth's researches are of special value in this connection, and appear to place the experimental evidence of this important point on a firm basis, and explain the production of white thrombi. In the rapidly circulating blood, the central portion of the vessel is represented by a dark line in which you see no corpuscles whatever; nothing but a red streak, on either side of which there is the so-called still layer, with an occasional leucocyte. This represents the blood current in its active rapid condition. If the circulation becomes slower, then it is seen that, in addition to the leucocytes which collect in the still layer, the blood plaques appear; but in the rapidly circulating blood, as seen in the mesentery or the omentum of the guinea-pig or the rabbit, the still layer, the peripheral portion, contains no blood plaques, and only occasionally a leucocyte—in fact, the corpuscles are separated from the wall of the bloodvessel by a distinct tube of plasma.

Eberth brings forward these facts in explanation of the development of white thrombi. So long as the circulation is active the plaques remain central, and adhere neither to each other nor to the vessel wall; but when, from any cause, the current is slow, this natural disposition of the corpuscles is disturbed, and the plaques tend to collect at the periphery, and aggregate in groups at any point which has been injured, or which has been deprived of the endothelium. Slowing of the blood stream is then, on this view, one of the essentials in the formation of white thrombi, and this is entirely in accord with what we know of the pathology of these structures. It is not alone the presence of intact endothelium which prevents the formation of thrombi in the vessels, for we frequently find in aneurisms, on the heart valves, and on the aorta, denuded and rough regions upon which thrombi do not form. Indeed thrombi are not often found on atheromatous ulcers, which would offer the favorable localities for their formation if it is the epithelium alone which prevents it. The other condition would appear to be slowing of the blood stream, which has long been known to play such an important part, and the true significance of which is well seen in the light of these observations of Eberth.

What I contend is, that the white thrombi are composed chiefly of plaques, and that the colorless corpuscles play an altogether insignificant part in their formation, and the experimental evidence which has been offered is borne out completely by a study of morbid anatomy.

The further development of the thrombus results from the disintegration of the plaques, and the formation of a finely granular material in which there may be no fibrin filaments. We must recognize a granular or stroma fibrin, as Landois call it, and a fibrillar or

plasma fibrin. The former is a granular material which develops when cells undergo the peculiar metamorphosis described by Weigert as coagulation-necrosis, and it is this in reality which goes on in the white thrombi. There may be no trace of fibrin filaments, but the chief mass is made up of a granular matrix in which the outlines of the plaques are no longer visible. The stages of this transformation I have traced in thrombi of the femoral vein, and it is well seen in passing from the superficial parts to the deeper parts. The plaques on the surface of a white thrombus, as at Fig. 5, may be intact, or they may show signs of disintegration and conversion into a granular débris. The central softening of a white thrombus results from the liquefaction of the plaques, and is a result possibly of the presence of fluid in greater abundance than is necessary for the process of coagulation-necrosis. Quite recently in a case of typhoid fever, I had an opportunity of studying the histological characters in thrombi in the femoral veins. In both they were mural, and had originated behind the valves. The attached portion was a light brown-red color, but the upper half was of a dead-white color, and the extension into the iliac was of the same character. The line of demarcation between the two parts was pretty clearly defined. At the thickest portion the superficial white thrombus had softened to an opaque milky liquid, but at the prolongation it was firm and consistent. A few colored and colorless corpuscles were scattered through the white thrombus, but the great mass of it was composed of blood plaques, and a study of the softened milky region showed clearly that the granular detritus was composed of the altered plaques. In the deeper parts the plaques became less and less distinct, until a point was reached at which the individual cells were no longer visible, and there was nothing but an indifferent matrix. The contrast in color between the outer and inner portions indicated a difference in age, possibly in mode of formation, though in the outer portion of the brown and the inner part of the white, close to the line of demarcation, the structure seemed identical.

In the light of these new observations on the connection of the blood plaques with thrombi, the entire question may be restudied with advantage, particularly the relation of the white and mixed thrombi, and the mode of formation of the clot in aneurisms. Of the truth of the statements here made regarding the connection of the plaques with thrombosis, I feel assured from careful observation on the structure of the white thrombi, (1) on atheromatous ulcers, (2) on the valves of the heart, (3) in aneurisms, and (4) in thrombi of the veins. I have not lately had an opportunity of examining a "globular vegetation" of an auricle or ventricle, but I venture to state that they are composed originally of similar structures.

ORIGINAL ARTICLES.

THE ANALGESIC ACTION OF THEINE.¹

BY THOMAS J. MAYS, M.D.,
OF PHILADELPHIA.

NEARLY a year ago I essayed to investigate the physiological action of theine, caffeine, and guanine, principally in relation to the nervous system,

¹ Read before the College of Physicians of Phila., April 7, 1886.

and from the experimental results which I then derived, and which were published in *The Therapeutic Gazette* for September, 1885, I was led to believe that, while all these agents had a decided affinity for the nervous system of the frog, caffeine chiefly affected motion, and theine and guaranine mainly influenced sensation. I carried this principle of the action of theine into practice, and found that this theoretical deduction was verified by its power of promptly relieving painful affections of the human body.

These clinical observations on theine appeared in *THE MEDICAL NEWS* for December 12, 1885. At that time I was under the blissful impression that these alkaloids were manufactured separately from tea, coffee, and guarana, but soon found that this delusive charm was only a snare—that they were neither the one thing nor the other, but that they were made indiscriminately from Chinese tea, Paraguay tea, kola-nut, coffee, or guarana; and were labelled promiscuously to accommodate the demands of the trade. This singular state of affairs has brought about a remarkable complication in the market. The great bulk of what is supposed to be caffeine, is not caffeine at all, but theine; since under all ordinary circumstances it is much cheaper for the manufacturer to work with tea, than with any of the other raw materials.

On this subject, Dr. Charles Rice, than whom there is no better authority, in an editorial in the March number of the *American Druggist*, says:

"We have reason to believe that neither theine nor caffeine is manufactured in this country, but all that is sold is imported from abroad. . . . We translate the following passage from Von Gehe & Co.'s *Handelsberichte*: 'Although caffeine is in fair demand, and still remains at its comparatively low figure, an advance is to be expected because the proper crude material for its preparation, namely, the cheaper grades of tea, have been for some time unobtainable.' . . . From this it appears that cheap tea leaves are the principal source of caffeine, and, therefore, also of theine.

"We are, moreover, assured by the house of E. Merck, of Darmstadt, that in view of the heretofore acknowledged and undoubted identity of caffeine and theine, manufacturers consider themselves at liberty to choose whichever crude material happens to be cheapest in the market. A lot of coffee, damaged by water or otherwise, would probably sometimes form a cheaper source than tea leaves. Usually, however, the latter are used as the source."

The opinion of Dr. Rice, that no theine or caffeine is manufactured in this country, I can confirm so far as this city is concerned; for inquiry among the principal dealers and manufacturers of drugs has failed to reveal any who did not import these alkaloids.

In all fairness it must be admitted, however, that the manufacturers were justified in producing and mixing these drugs promiscuously, since they are taught that they are identical both in chemical constitution and in therapeutic action, and if this is true, no earthly reason can exist for their separate manufacture. But it is very evident, that for the purpose of inquiring into the truthfulness of their

supposed identity, it is idle to test them in a state of combination; hence, in so far as my former experiments are to indicate any differential action between these agents they are valueless. There can, however, be no doubt that what I used under the name of theine was principally or wholly theine, but the nature of the substance which I regarded as caffeine is more uncertain, yet it must have contained something besides theine, or else I could not have obtained dissimilar results from the two substances. In this connection I would also state that neither the theine nor caffeine which I then used came directly from Merck, as I had supposed at the time, and erroneously stated in my first article.

In view of this chaotic condition of things, I determined to review all my previous work, so far as it related to theine and caffeine, and my thanks are due to Mr. William Harris, with the firm of Messrs. Henry C. Blair's Sons, for isolating these alkaloids from tea and coffee respectively, and for thus enabling me to test each one in its separate state. I have thoroughly gone over my former ground, and have found ample proof to substantiate my previous investigation in most essential points, and even have found reason to believe that the difference between the action of the two in regard to sensation is greater than my earlier experiments indicated.

Since tea and coffee are believed to be very different in their gross effects on the human body, there is no obvious reason why their alkaloids should be considered identical in their action, unless, after being shown that they are chemically alike, the assumption, of course, naturally follows that they are alike in their physiological action. This idea has also been encouraged by experimental physiologists, among whom are Aubert, Albers, Amory, Bennett, McKendrick, Burnett, and others, although Leven, who, as early as 1868, experimented with the separated alkaloids, which it does not appear the other above mentioned experimenters did, found that theine produced convulsions in frogs, while caffeine did not. This fact my experiments confirm, and must be regarded as an important indication as to the manner of their action.

A detailed account of my experiments just completed will not be given here, since they would swell this paper to inordinate dimensions; but their condensed results, which appear below, show that, while the actions of theine and caffeine agree in many respects, they also disagree in some important particulars. They both first affect the anterior extremities of frogs, and both diminish respiration and cause hyperæsthesia during the latter stage of the poisoning process, although the supersensitiveness is much more marked in theine than in caffeine. They differ in the following respects: Theine principally affects sensation, while caffeine principally influences motion; theine produces spontaneous spasm and convulsions, while caffeine does not; theine generally produces muscular relaxation in the posterior extremities, while caffeine produces muscular rigidity in the same; theine impairs the nasal reflex early in the poisoning process, while caffeine does not, if at all, until in the very last stage.

It may be remarked here that since winter frogs, which are not so irritable as summer frogs, were used in my last experiments, these results, so far as the induction of hyperæsthesia is concerned, may be modified to some degree; but so far as sensation is concerned, which is the main point of interest here, I am confident that the distinction made above between the action of theine and caffeine rests upon a fundamental basis. It would be erroneous, however, to suppose that the action of theine is strictly and exclusively limited to the sensory nerves, and that of caffeine limited in the same way to the motor nerves, for both my former and later experiments teach that these are merely their strong characteristics, although one may supplant the action of the other in a minimum degree.

On man, theine can only be considered a poison in large doses. Frerichs, Lehmann, Husemann, and others drank it, and found that in doses of from one-fourth to one-half of a decigramme, it produced "frequency of the pulse, irritation of the bladder, cerebral excitement, slight hallucinations, and lastly a desire to sleep." Dr. C. H. Castle, of Cincinnati, Ohio, was induced to test the action of theine by the appearance of my article in *THE MEDICAL NEWS*. The results of his experiments, which were both physiological and clinical, were read before the Cincinnati Medical Society, and published in *The Cincinnati Lancet-Clinic* for February 6, 1886, and since they were made with pure theine, extracted by his own chemist, and because they add some hitherto unobserved features of the drug, they are very interesting and instructive. He made, in all, three experiments upon himself, and of these he says:

"The first time I injected one-sixth of a grain into my left forearm, immediately below the flexure of the elbow-joint. I will not weary you with the details of the observations, as made at intervals of a few minutes. The noticeable effect was remarkably rapid. An angry blush and swelling spread from the point of injection, but what looked like a most delicately tender spot was singularly devoid of sensation. It was not, nor did it become, totally anæsthetic, but, in what appeared to be an inflamed area, sensibility was markedly dulled. Presently a numbness such as we feel when a member is beginning to 'go to sleep,' from pressure upon its supplying nerves, stole over my wrist and hand. Little tinglings, scarcely noticeable had not one been on the watch for strange manifestations, came and went in various places. At no time was mobility impaired. The temperature of the left hand, and I beg to call your attention to this point, was markedly lowered. The effects of the injection lasted about two hours.

"The second injection was made under somewhat different circumstances. I had rather severely bruised my right hand, and the pain on pressure over the metacarpal bone was quite severe. I therefore injected one-third grain of theine subcutaneously, over the ulnar nerve just below the elbow.

"The same phenomena were noticed as in the first use of the alkaloid—sensibility around the injection was markedly diminished, lower down the forearm and in the wrist and hand less markedly so; the right hand lost as much in temperature as had the left hand previously, but I am compelled to say that I was disappointed in the analgesic effect that I had expected. The extreme sensibility was dulled, to be sure, and my hand no longer hurt when I put it into my overcoat pocket,

as it had before, but any deep pressure instantly informed me that the nerves about the joint still resented the impact of a foreign body with the surface.

"The third observation was made again upon the left arm; this time one-half of a grain was used, and though an accident perhaps somewhat marred the experiment, some points were brought out more prominently. The injection, made for me by a friend, was a deep one—intermuscular—and the withdrawal of the needle was followed by a slight hemorrhage, sufficient to raise a conical subcutaneous tumor of a diameter of a one cent piece. Instantly there was a deep-seated pain, probably due, I thought, to extravasation from the wounded vessel into the intermuscular planes. Full extension was impossible, partial extension very painful, the painful seats being deeply under the point of injection and the dorsal aspect of the metacarpo-phalangeal joints.

"As in former observations, sensibility was affected almost immediately, and in three minutes the left hand was blanched and almost cold to the touch. My pulse was always full, strong, and but slightly more frequent than normal. Very soon peculiar constitutional disturbances appeared, and not entirely like those described in the standard works on *materia medica*. I was excited and talkative, and so rapidly did I talk, that I would soon exhaust a subject broached by others, and endeavor to introduce some natural descendant of the idea as a topic of conversation, apparently so far ahead of its proper sequence in a well ordered-train of thought, as to appear like an interruption with an irrelevant subject. Failing to gain a hearing, as I frequently did, I would indulge in snatches of song or broken ejaculations which had a world of meaning in them then, but which, as I look back upon them now, seem utterly absurd.

"Alternating with states of great bodily activity were spells almost of a fainting character. These were seven or eight in number, and beginning soon after the injection, gradually ceased after the lapse of six or seven hours."

Dr. Castle gives a very good description of the theine effects as he observed them on himself, and I would say that the only way in which I can account for the decided cerebral intoxication in his last instance, is on the score that unfortunately the deep injection which he practised chanced to introduce the theine into a bloodvessel, and it was at once carried to the brain and produced the hallucinations as described. This is all the more probable because the central disturbance manifested itself very soon after the injection, which, ordinarily, should not have appeared until in a later stage of the poisoning process. The dose which he used, I should now, after considerable experience with the drug, call a moderate one—for I frequently inject from three to four times as much as he did—not deep, however—and I have yet to observe the first untoward constitutional effect.

The physiological effects of theine as brought out in a number of individuals, under my own observation, may be summed up as follows: Dose, one-third of a grain. Numbness of arm and hand below seat of injection, "a feeling as if the hand had been steeped in a solution of carbolic acid," as one of the subjects of experimentation expressed it. A feeling of coldness and an occasional disturbance of the temperature in the member under its influence. A slight reduction in the pulse-rate, and no intoxication of the brain. No impairment of motion. The anæsthesia comes on in a very few minutes after the

injection, and was much more marked in some individuals than in others. The temperature disturbance was not regular. Out of four cases in which the temperature was taken, it showed no difference in one, and in another one the uninjected hand was slightly higher (0.4° Fahr.) in temperature than the injected one. In two there was quite a marked fall in temperature—one 0.8° Fahr. and the other 1.2° Fahr. lower in the hand of the injected arm than in that of the uninjected one. And strange to say, the hand which showed the greatest depression in temperature, experienced the least anæsthesia. A certain feeling of coldness always accompanies the numbness of theine, yet, in my earlier observations I failed to detect any difference with the thermometer. Dr. Castle was the first to notice this feature, and I can quite agree with him, that if the lowering of the temperature is constant after its injection, this will add materially to the clinical worth of the remedy.¹

It is evident now, I think, that experimentation points out that in theine we have an agent which has the power of paralyzing sensation without affecting motion, with a great degree of accuracy, and this with no appreciable injury to the part which is influenced. It now remains with clinical medicine to decide how far this property can be utilized in practice. Evidence from various sources will, however, be offered here, which seems to show that it has already demonstrated its fitness to fill a most useful position in the treatment of pain. The following cases, the sources of which will be indicated during their relation, have all been gathered from different medical practitioners who took sufficient interest in this drug to test its action, and who were kind enough to transmit their reports to me; and I desire to thank these gentlemen for the scientific and liberal spirit which they evinced in undertaking to investigate a hitherto untried remedy. Both the successful cases and the failures will be given, in order to form an intelligent verdict in regard to its proper place in the treatment of pain.

The first series of cases were reported by Dr. H. C. Fegley, of Ashland, Penna., who writes:

"Since your article on the action of theine was published, I have had the opportunity of testing its analgesic effects in three very aggravated cases of sciatica. In two cases complete recovery, with the exception of slight stiffness of the leg in one, took place. The third was an aggravated case of three years' standing, and although not cured, was so much benefited that she is able to attend to her household duties."

CASE I.—S., December 20, 1885, had been unable to leave her chair, in which I found her, for two weeks, the least movement causing the utmost agony in the whole left leg. When perfectly quiet no pain was felt except occasional sharp twinges shooting down to the foot. I put her to bed and injected a quarter of a grain of Merck's theine into the thigh, about the seat of greatest pain. The injections caused intense burning, which

lasted about three minutes, when she said it had left her and that movement in her limb caused less pain. The following evening I injected half a grain, and repeated the dose a day after, when she felt, as she expressed it, entirely cured.

CASE II.—H., January 10, 1886, complained of intense deep-seated pain in the leg, which had been coming on gradually for nearly a week. She never had sciatica before, and two injections of three-quarters of a grain each relieved her entirely.

CASE III.—Mrs. A. has been a sufferer for nearly three years, and has tried a great many remedies with little or no benefit. Theine in doses of a fifth of a grain relieved her only for three or four hours, but three-quarter grain doses gave her a full night's rest. The intense sciatic pain, she says, has passed away, but there are still some soreness and considerable stiffness in the limb.

I found that the dose you first suggested to me was inadequate, so I first tried one-fourth, then one-half, and finally three-fourths of a grain, and am quite well pleased with it. I have only had an opportunity of testing the drug in the three cases just cited, but am inclined to believe it to be a true analgesic in aggravated forms of neuralgia.

Reported by Dr. H. Woddrop, of Loag, Pennsylvania.

CASE IV.—M., married, aged forty, was found last December 4th, with considerable pain, of intermittent character, in left leg. For a month previous he had been suffering with occasional twitches of pain, and was now confined to his couch. He also had stiffness as well as œdema in his other limb. He was treated with morphia (*per orem* and subcutaneously), muriate of ammonia, quinine, iron, strychnine, and arsenic until the last of December, when the swelling had entirely disappeared, but there were considerable pain and stiffness left in the limbs. About this time I saw your article on theine, procured some, and injected one-fifth of a grain deep into the calf of the left leg. This caused some pain, and succeeding symptoms were very near your description. Continued internal treatment and subcutaneous injection of theine occasionally. On January 19th changed internal treatment to small doses of phosphorus, beef peptonoids, and milk. On January 23d could move his limbs freely, had no pain, and appeared, excepting debility, a well man. He received in all twenty-four injections, sometimes twice a day. I attribute his recovery in great part to theine, but the constitutional treatment was also very important. I have no doubt that by the use of theine I shortened the case materially. The burning which was occasioned by the injection lasted about fifteen minutes, after which pain in the limbs ceased. The pain returned at varying periods, not as severe after the injections as before, and it always subsided directly after the use of the theine. I believe theine to be a perfect analgesic, and have noticed no narcotic effects from its employment.

CASE V.—Mrs. T., age between sixty-five and seventy years, a rather fleshy lady, complained of dyspnoea, excessive weakness; heart's action was muffled—the two sounds running into each other. Also had severe pain along the course of the right sciatic nerve. Injected theine over right sacral region, and left the syringe and solution of theine in charge of an intelligent lady, with instructions to use it immediately on return of pain. Also prescribed digitalis, ferri sulph. exsiccatus, and quinia. At my next visit I found the heart sounds and breathing normal, and sciatica gone. She had received, in all, three injections of theine.

CASE VI.—This was a case of rheumatic gout in which I used theine for the purpose of obtunding sensibility,

¹ After this paper was written, I learned from Dr. L. Wolff, of this city, that Dr. A. Eulenberg, in his "Hypodermatische Injection der Arzneimittel," describes the analgesic action of caffeine when administered subcutaneously. On referring to the work, I found that this eminent author had used caffeine for the relief of pain in three cases—two of cephalalgia, and another of occipital pain—and, although he was unsuccessful in relieving the pain, at least permanently, he observed that caffeine produced a local anæsthesia at and in the vicinity of the injection.

and it certainly appeared to have considerable influence upon the pain, but, of course, we could not expect any more in this case from it.

To case first I gave your article to read, and he said that his symptoms of numbness, etc., corresponded with those which you describe. The second and third cases, besides the burning incidental to the injection, experienced nothing more than the relief of pain.

Reported by Dr. George S. Gove, of Whitefield, N. H.

CASE VII.—Injury to the hip-joint of one and a half year's standing. The theine was used at first in small and afterward in large doses without any benefit, either alone or combined with morphine and atropine. Nor had morphine or atropine, combined or alone, any influence on the pain.

CASE VIII.—Patient aged forty-five, employed in a creamery. Has had rheumatism. Brought on a pain in his back by lifting a heavy milk can. Worked all that day, had a restless night following; next day was quite lame and stiff, worked that day but was used up by night-time. The following night of the second day he had very little sleep on account of the severe pain. Liniments and hot fomentations were applied without effect. When I saw him next morning the pain was so great that in trying to dress himself he fainted and fell to the floor. I gave him one-half grain of theine hypodermatically, about ten inches above the hip-joint, and waited half an hour without affording much relief. I then gave him another half grain injection and the pain lessened. After a short time the pain entirely disappeared and has not returned since.

Dr. William Hall, of Conshohocken, Pa., reports:

CASE IX.—I have used theine in three cases, two of cervicobrachial and one of intercostal neuralgia. The case of intercostal and one of cervicobrachial neuralgia were entirely relieved. In the other case theine failed, as did all other remedies. This last case is a woman whose father died of, and whose sister is suffering from, a similar form of pain. I am well pleased with the remedy in a fair case.

Dr. G. D. Bennett, of Newton, Kansas, reports:

CASE X.—In regard to my patient treated with theine, I would say that Mr. G. fell from his wagon in the early part of November, 1885, and dislocated his shoulder downward into the axilla, and also injured the brachial plexus of nerves in the fall. The latter was followed by some neuralgic pain, which grew to such severity that one-half grain doses of morphia gave him but partial relief. Some six weeks after receiving the injury and when the pain in the shoulder was steadily growing worse, I commenced using theine hypodermatically by injecting one-third of a grain in the morning. The result of the first injection was some redness and pain at seat of injection, and in fifteen minutes numbness of finger-ends appeared, which gave way in half an hour to a condition explained by the patient as "arm being asleep." In a short time, probably forty minutes, pain in shoulder all gone. Next day visited patient and found return of pain, but not so severe as before, and again injected one-third of a grain, which relieved him to such an extent that he had a comfortable day and an easy night. This treatment for the next twenty days was combined with tonics, at the end of which time my patient had complete use of his arm and no pain. I feel that we now have a remedy which is certainly far superior, at least was in this case, to morphine and cocaine, for they both had been tried thoroughly.

Dr. C. H. Castle, in the article to which reference

has already been made, relates four cases of pain treated with theine as follows:

CASE XI.—The patient was the subject of those ill-defined, dragging musculo-rheumatic pains of the extremities, found not infrequently in such a damp climate as is ours. About one year ago he had obtained marked relief from an electric bath, taken in Chicago, and at the time I encountered him he was on a search for a similar electropathic institution in Cincinnati. I persuaded him to allow me to use one-sixth of a grain of theine upon him hypodermatically. The pain was severe in the anterior muscular masses of both thighs and in the calves of both legs. I made the injection slightly below the popliteal space and toward the inner side of the leg. The effect was striking, and almost instantaneous. The point of injection, though angry-looking, was quite anæsthetic, and as little tinglings shot down the leg toward the foot, the pain vanished and the analgesia of the injected leg was in marked contrast to the pain in the thigh of the same extremity, and the thigh and leg of the opposite extremity. This beneficial effect was maintained for four days, at which time I last saw the patient, and I exceedingly regret that we have neither of us since had the time or opportunity to relieve the still suffering fragments of his body of their aches.

The other case I will give from the notes taken by the attendant physician:

CASE XII.—Myalgia of the deltoid muscle. Pain severe. Had had one-quarter grain of morphia every two hours the night before without bringing sleep. Could not raise arm.

9 P.M. One-sixth grain of theine subcutaneously about the anterior margin of the trapezius, just above the shoulder. Pulse 102.

9.04. Very little pain, unless she moved her arm. Still marked tenderness on pressure.

9.05. Pulse 96. Diminished sensibility about seat of injection.

9.10. Seat of pain seemed to be only in lower half of its former area.

9.13. Numb feeling in arm as far down as elbow. Very little pain on moving arm.

9.15. Can now bear pressure over former exquisitely tender spot.

9.20. Pulse 96. Only tenderness is now over acromion. No pain at all on pressure over an erior part of arm.

9.30. Numbness not so marked. Says if only had as much pain as she now has she could easily bear it.

9.37. Pain now only about insertion.

9.40. Numbness in arm diminishing, but pain not increasing. Says she could go to sleep.

11. Pain has returned, but in a much less degree. Needs no morphia.

Since then has not complained.

CASE XIII.—Neuralgia of the sciatic nerve. Chloroform, ether, etc., had failed as curative remedies, or even to relieve pain. Temporary relief was being sought with morphia.

4.05 P.M. Injected one-third grain of theine over the course of the sciatic nerve near its emergence from the pelvis.

4.10. Feels easier below the seat of injection. Still tenderness on pressure over the nerve.

4.20. Still somewhat better.

4.25. Jerking sensation when walking, no pain while lying in bed.

4.35. Tingling sensation in heel, walks much better.

5. Numbness from knee down to foot; warm flashes.

5.30. Same feeling as at 5 P.M., with pain in hip.

Pain not so severe in hip; numbness from knee down.

6.30. Complains of foot feeling very cold. It communicates this sensation to the hand.

7. No pain, but still a sensation of cold. Hot sand bags applied.

Next morning pain had returned.

CASE XIV.—In this case there was absolutely no benefit derived whatever. The pain was not at all relieved.

It would be highly satisfactory if the nature of this last case of pain had been given by the author. In the absence of these important data, it is, of course, useless to speculate on the probable cause of its failure to act.

Dr. Washington H. Baker, of Philadelphia, reports the following cases:

CASE XV.—L. N. W., aged thirty-six, consulted me December 13, 1885, on account of a pain in his left hip, from which he had suffered for eight months. Never had any disease of the genitalia. Has not had chills and fever, nor rheumatism. Had an attack of typhoid fever thirteen years ago, and been well ever since until the present trouble appeared. The greatest pain complained of is in the left hip a little above the trochanter major, the pain also radiates around in front and down back of thigh. The pain does not extend below the knee at present. Some time ago the pain extended to ankle. Tongue clean. Bowels regular. Appetite good. Is compelled to use a cane and limps when walking.

Dec. 16. Gave a subcutaneous injection of twenty minims of a two per cent. solution of theine. He felt better at once and could stand straighter. In a few minutes had a tingling sensation in thigh. Can walk more erect and with less pain. The thigh feels numb when touched, the leg less so. Half an hour after the injection the thigh still felt numb, but he could not walk quite so well. Was given five grains of iodide of potassium thrice daily. The injection was made about three inches above the seat of pain.

17th. Injected twenty minims of theine. Helped him somewhat, but not a great deal. Applied a thapsia plaster to thigh.

18th. Tells me he had less pain since yesterday, than since he had the disease. Injected twenty-five minims of theine.

19th. Walked down to office this morning, a distance of twenty squares. Is feeling much better. Injected thirty minims.

20th. Feels about the same as yesterday. Little or no pain. Injected thirty minims.

21st. Feels better than he has yet, less pain than yesterday. Injected twenty-five minims.

24th. Yesterday morning the pain returned in the limb. Last night it was as bad as ever. Injected twenty minims, and after three minutes the pain began to grow less.

25th. Still considerable pain. Injected twenty-five minims.

26th. Had severe pain last night. Injected twenty-five minims.

28th. Still considerable pain. I gave a pill of sulphate of cinchona, ext. nux vomica, ext. belladonna, and pil. ferri carb.

Jan. 6. Feels better. Injected twenty-five minims of theine.

30th. Walked into office to-day, erect, very little pain complained of and a slight limp. Considers himself cured.

CASE XVI.—J. B. came to me in December, 1885, complaining of a severe pain in lumbar region which almost incapacitated him for work. Injected twenty-five minims of a two per cent. solution of theine in lum-

bar region above the painful part. He was relieved at once and has not been troubled with it since.

CASE XVII.—J. R. W., aged thirty years, brakeman, suffered from a pain in the lumbar region which troubled him particularly when he bent over to wash his hands. I injected twelve minims of a two per cent. solution of theine under the skin above the painful spot with immediate and entire relief of pain. It was really ludicrous to see the astonished expression on his countenance and the contortions he went through to assure himself that he was in truth relieved. I also gave him two and a half grains of iodide of potash thrice daily.

Dec. 22. Injected twenty-five minims of theine, as there was a suspicion of pain in the back. This visit was one week after the first.

Jan. 12, 1886, reports no trouble with his back.

CASE XVIII.—March 8, 1886, a gentleman, fifty-eight years old, told me that he had been suffering for several weeks with a pain in the left shoulder. He supposed it was rheumatism. He had been kept awake the greater part of the last three nights on account of the pain. The pain was getting worse from day to day. I injected twenty-five minims of a two per cent. solution into the shoulder with immediate relief. That night he slept comfortably, and has not had any pain in shoulder up to date (March 20, 1886). After the injection a burning, tingling sensation was complained of at site of injection. The theine had no appreciable effect on the mental faculties.

CASE XIX.—March 20, 1886, a patient complained greatly of a bunion on right foot. The pressure of the shoe in walking caused considerable suffering and a slight limp. Fifteen minims of a two per cent. solution of theine were injected about three inches above inner malleolus. Four minutes after injection the bunion could be roughly stroked and pinched without discomfort. The shoe was put on, and walking caused no pain whatever.

Dr. J. H. S. writes from Bolivar, N. Y., under date of December 21, 1885, about his own case as follows:

CASE XX.—“Dear Sir: I have just this day read your article on the ‘Therapeutic Action of Theine,’ and would say that my own case is almost a *fac simile* of your case, Mrs. A. A. (which was a case of sciatica). I have tried every remedy I could think of, but have found nothing to give relief but injections of morphine. I had to give up the morphine on account of the constitutional effect which it brought on. My trouble was caused by being thrown from my cutter about a year ago, but had no severe pain until last August, when I became a subject of severe blood-poisoning contracted during an operation, and from which time the pain in my limb began to be terrible. I went to the springs and continued treatment for the poison. I feel so badly now that I find it impossible to write you in full. If you will be kind enough to send me some theine, with full instructions how to use it, I will test it and have my case carefully reported to you. I would further say that I returned from the springs about three weeks ago, but received no benefit. I am now and have been in bed for the past ten days; and I only hope this new remedy will give me some relief from the most terrible pain possible to endure.”

Under date of March 22, 1886, Dr. S. states:

“In relation to the theine, I will say that it has done its work for me. The first injection was made on December 25th, one-third of a grain in the morning, and one-third of a grain more in the evening. Then I followed this up with daily two-third grain doses for six days,

then two doses of the same amount for the next three days, then two doses in five days, and the last one in six days afterward. After this I was free from pain. My leg, which was three inches smaller than the opposite one, is now about its natural size. I am a thousand times your debtor for the theine you sent me, for I have full confidence that it gave me relief, and, so far as my sciatica is concerned, it made a cure without any bad effects.

CASE XXI.—I have tried theine in two other cases, but was not able to watch the effects as I would have done had I been in condition, but can say one was a strong man with rheumatism of the left arm. I gave him two injections of two-thirds grain each, which relieved his pain.

CASE XXII.—The other was a man of about fifty years old, with sciatica, which had troubled him for years. I gave him two-third grain doses once a day for three days, which gave him incomplete relief. I then gave him a grain a day for two days, and this gave him entire relief. I will say, however, that this case I have not seen since I gave him the last injection.

Since the appearance of my last article on this subject in *THE MEDICAL NEWS*, I have treated quite a number of cases of pain with theine, of some of which I beg leave to give a condensed report in this connection.

CASE XXIII.—Mrs. B., aged thirty, was entirely relieved of intercostal neuralgia, accompanied by a peculiar constant burning pain in the left interscapular space, by one injection, which was given over painful spot along left side of spine.

CASE XXIV.—Dec. 5, 1885. V., chronic rheumatic pain of left shoulder-joint and whole arm. Pain very severe. No relief rendered previously by salicylates, muriate of ammonia, iron, quinia, or iodide of potash. One-half grain of theine relieved the pain at once and made him feel comfortable.

CASE XXV.—Dec. 17, 1885. Mrs. W., aged fifty, suffered from cervicobrachial neuralgia on right side since the previous August. Two injections of half a grain each, a week apart, were sufficient to relieve the pain.

CASE XXVI.—Dec. 29, 1885. M. C., aged forty-seven. Pain in and inability to move left arm for three weeks. Unable to lie on left side. Could not dress himself without aid. Pressure along the region of the left brachial plexus greatly intensified the pain along the shoulder and the arm. Injected half a grain over seat of pain along left side of spine, and in less than five minutes he was able to lift his arm to his head, to swing it to his back, and, very much to his surprise, to dress himself—something which he had not been able to do for three weeks. He said he was entirely relieved. Have not seen him since.

CASE XXVII.—January 7, 1886. Case of double sciatica of fourteen years' duration. Theine injections relieve the pain, but are powerless to cure the degeneration which has probably taken place in the nerves.

CASE XXVIII.—January 12, 1886. Sciatica in left leg relieved by two injections.

CASE XXIX.—January 12, 1886. K., aged forty-five. Burning pain in right interscapular region, which was entirely relieved by one injection over the seat of pain.

CASE XXX.—February 9, 1886. H. was cured of a brachial neuralgia by one injection.

CASE XXXI.—February 18, 1886. Pain in the lumbosacral region; also complains of pain in left brachial plexus. Gave him two injections—one over the former and the other over the latter seat of pain, and he has not complained since.

CASE XXXII.—December 16, 1885. Severe case of sciatica, of two weeks' duration, was relieved by seven injections.

CASE XXXIII.—February 18, 1886. Intercostal pain aggravated by pressure along left side of spine in upper dorsal region. One injection over latter place cured him.

CASE XXXIV.—February 3, 1886. W. This was a most aggravated case of neuralgia of both lumbosacral and left cervicobrachial plexuses, associated with irritability of the spine along its whole course. The pain had been coming on for two months or more. It was intermittent, very intense, and accompanied by muscular contractions of both legs below the knee and left forearm. It was impossible for patient to stand erect when he suffered from the pain—in fact, he was confined to his bed most of the time. Four injections, of one-sixth grain each, in the evening, made him comfortable during the night and next day until evening, which, with other remedies, like iodide of potassium, iron, muriate of ammonia, and quinia, have been kept up for nearly three weeks; and now the patient is in a good condition, able to walk twenty-five squares in one stretch, and only requires an occasional injection. The injections were all made on each side of the spine, in the interscapular and lumbosacral region, and never failed to give him instantaneous relief, no matter how bad the pain was at the time.

CASE XXXV.—M., æt. twenty. Carbuncle in posterior part of right thigh, from the pain of which she suffered intensely. Injected one-third grain of theine about three or four inches above the seat of pain, and she became easy from that time on until it was lanced. She is accustomed to the pain of carbuncles, and says that the injection gave her immediate relief.

I might multiply these cases, for I have treated a number more in the same way, but I presume that what has already been given, especially the history of those cases given before my own, is sufficient to show that theine is of considerable clinical value. In addition to the theine treatment, I will say, that all my aggravated neuralgiæ had received iron, quinia, ammonia, iodide of potash, and arsenic. For it must be remembered that the function of theine is solely that of pain-relieving, the intimate nature of which is not understood; and while it performs this office to perfect satisfaction, it also fails to give permanent relief to obstinate and protracted cases of pain unless it is combined with remedies which assist in changing the nutritive state of the affected nerves.

The question now arises, Is theine an analgesic in all kinds of pain? This is best answered by referring to its physiological action. I think my experiments on frogs demonstrate that while it reduces sensation when locally applied, its chief characteristic action begins at the spinal cord and spreads thence to the periphery of the nerves. Hence it is indicated in all pain of central, especially of spinal origin, as in that of the different forms of neuralgia, of neurasthenia, of locomotor ataxia, of ischæmia of the spinal cord, and of spinal irritation. I have also seen it act very well in lumbago. But pain depending upon peripheral irritation, such as that caused by an injury to the surface of the body; or that caused by a carious tooth, etc., is more successfully treated by the surface or subcutaneous application of cocaine.

The action of cocaine is, in many respects, the inverse of that of theine. It does not, like theine, produce anæsthesia of the skin or mucous membrane by affecting the trunk of the nerve and thence all its terminal filaments, but by coming in direct contact

with the endings of these filaments. It is for this reason that cocaine has failed as an analgesic in neuralgia. I do not wish to assert that cocaine is incapable of affecting the nerve trunk in the same way as theine does if it were injected deep enough to bring it in direct contact and then confined there, or, at least, below the seat of injection, by constricting the circulation of the limb, as has lately been suggested by Dr. Corning, of New York; but when cocaine is injected subcutaneously in the ordinary way its transient anæsthesia will be limited chiefly to a small circumscribed area at the seat of injection, and will not affect the nerve trunk beneath.

This distinction is important, for it shows that theine, while it is a local anæsthetic in one sense, is not so in the same sense as is cocaine. The former when injected subcutaneously renders a whole or a part of a limb numb, but it is powerless to reduce sensibility to the same degree as cocaine does when applied locally. The fact that theine chiefly acts from the centre to the periphery, makes it very important, in a practical point of view, that it be introduced above the seat of pain, or, at least, over the central origin of pain, in order to secure its full analgesic influence.

The localized action of theine brings out one of the advantages it possesses over morphine and other analgesics. It is pretty well established that morphine, narceine, and other agents of the same class, bring about analgesia only by intoxicating the brain and higher nerve centres, and leaving the peripheral nerves more or less uninfluenced. This undesirable feature of morphine action is absent in theine, for it seems to leave the brain entirely undisturbed, at least so far as any narcotic influence is concerned, unless given in very large doses.

Theine, as a rule, is surprisingly prompt in exerting its analgesic action. I have on a number of occasions witnessed patients who had too much pain to move the arm, or too stiff to stand erect, swing their arms and straighten their bodies in less than five minutes after its introduction. It is not always so prompt, but this may be due to the smallness of the dose. I have seen cases in which no benefit had been derived from one-third of a grain, but which, on the dose being doubled, improved at once. Therefore, when a single dose fails to act favorably, it is always advisable to increase it, even if this has been done to a large extent. I have repeatedly injected two grains, not in a single, but in four separate injections, in as many different localities along the back, within the space of five minutes, and found nothing but the best results.

Another feature which recommends theine to favor is its prolonged influence. After pain has once been relieved by it, it is very rare to find the same returning inside of ten or twelve hours, and oftener not until in twenty-four hours, and then, in all probability, not in its original intensity. I am referring now to obstinate cases of pain. Where the pain is more unstable, it is not a seldom occurrence to find that one or two injections relieve it permanently.

In order to obtain the full effect of theine, it is not necessary to inject it deeper than immediately

beneath the skin. Of deep injections I am not able to speak intelligently, always having been satisfied with the results derived from superficial injections. Dr. Castle, in the article referred to, relates his own personal experience with one deep injection, in which the drug certainly exerted a marked influence on the brain, owing, probably, as has already been stated, to the fact that the whole dose received direct entrance into the circulation, and was at once carried to the centres of innervation.

No prolonged irritation and no inflammation are produced at the seat of injection. It gives rise to considerable pain or burning at first, but this disappears in the course of a very few minutes, and is replaced by a marked area of anæsthesia. Most of the burning is undoubtedly due to the comparatively large amount of water which is necessary to dissolve a drug of a solubility as low as that of theine in cold water—which is one in fifty parts. This can be overcome by dissolving the theine in warm water, in which it is very soluble, even at the normal temperature of the body. In this way a whole grain can be dissolved in a very few minims of water.

In regard to the employment of a particular kind of theine, I would say that it is tolerably certain that everything which falls within the commercial description of both drugs is principally made up of theine: hence very good clinical results are obtained from that which is found in the market—especially that of Merck, which I have principally used—although it is very obvious that, if my experimental deductions in regard to the differential action of theine and caffeine are correct, much better results are obtained from theine when unalloyed with caffeine.

In conclusion, I beg to say that all the practical value which the contents of this paper may have, emanates from the pharmacological laboratory, for it was through its methods alone that the first clew to the analgesic action of theine was discovered. I therefore trust that you will receive it as a gift from the experimental department of medicine to and in the interest of that science which we all delight to honor and to encourage, viz., internal therapeutics.

1716 CHESTNUT ST., PHILADELPHIA,
March 24, 1886.

A CASE OF RHEUMATISM WITH AN UNUSUAL AND ALARMING COMPLICATION.

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THE following notes place upon record the history of a complication occurring in acute rheumatism, which, so far as I know, has not yet been fully described.

On the 19th of April, 1885, I. P., æt. forty, a merchant, was attacked with sharp pain between the shoulders and in the right hip. The patient is a thin but muscular man of highly nervous temperament, and was at one time a well-known athlete. Has always been in very good health and of regular and temperate habits; cannot remember any pre-

vious illness, with the exception of one or two very slight attacks of so-called rheumatism, which did not necessitate rest in bed; and one, rather more severe, four years ago, which kept him in bed three or four days. No history or evidence of gout or syphilis. Father of the patient died young; mother and one sister at present in good health; never had any brothers.

The pain complained of was particularly severe in the upper and back part of the right hip, and was increased by pressure along the course of the great sciatic nerve; but pain between the shoulders, though present, was not severe. The general health was unaffected. I injected one-quarter of a grain of sulphate of morphia, but with little relief.

April 20 (second day of the illness). Slept badly and sweated profusely during the night. Pains between the shoulders, and in arms and wrists, but less in the hip than yesterday. No swelling in any of the joints. Does not feel so well; pulse 100, regular; temperature 100° F. in the morning; nervous and fidgety. Physical examination of the chest yields negative results. Fair appetite; coated tongue; bowels sluggish; urine scanty and febrile. Ordered salicylate of soda in twenty grain doses every four hours.

21st (third day). There was decided improvement. The pains less severe, the pulse reduced, the temperature normal.

23d (fifth day). The pains are not severe and not localized. Sweating profuse, more especially at night.

26th (eighth day). Is now fairly well; can move about the house, but still complains of slight pains about the chest. Reexamined thorax, but could find no physical signs to account for the symptoms.

28th (tenth day). Stopped the salicylate of soda mixture and gave one containing ten minims of colchicum wine and five grains of the iodide of potassium.

May 4 (sixteenth day). For the last two or three days has been obliged to return to bed suffering from pains in the joints, more especially of the wrist and the right thumb, as well as both sides of the chest, the groin, the spermatic cord, the left testis, the calf of the leg. The pulse has been 70 to 80; the temperature but slightly above normal. Urine acid; high colored; no albumen; no sugar. Appetite fair but capricious. Bowels sluggish.

Yesterday afternoon he had been more bright and cheerful than for some days, the pains about the body being less troublesome, but the sweating still profuse. Toward evening, however, he felt very much fatigued. At half-past seven felt sick, tried several times to vomit, but failed; finally threw himself back on the bed exhausted, saying that he felt faint and wanted air. A few minutes later I found him very much altered, though scarcely presenting the pallid appearance of one suffering from an ordinary syncopal attack. The breathing was steady and regular, respirations twenty to the minute, full and deep. The pulse beat strongly at 26; no irregularity or intermittency. The patient, though very weak and faint, was not unconscious. I lowered his head, slapped his face with a wet towel, applied a

sinapism to the heart, and warm bottles to the feet, all with the effect of considerably reviving him, but without quickening the heart's action, which fell in half an hour to 24 beats a minute. The breathing continued full and regular at 20-22.

At 8.30 P. M. Dr. George Ross joined me in consultation. At the time of his arrival the pulse beat was 24. An ether and ammonia mixture was administered, with no good result. At 9 P. M. the pulse fell to 22.

5th (seventeenth day). Slept fairly all night; taking nourishment at intervals. At 5.30 A. M., pulse 26; felt more comfortable and less weak. Having no stethoscope with me, I was unable to make a careful observation of the heart sounds during the night. This morning, however, there was visible pulsation in radial, ulnar, and carotid arteries. Apex beat was in its normal position; no thrill; heart's action regular; no increase in normal area of cardiac dulness. At the junction of the third left costal cartilage with the sternum, there is a soft, blowing, systolic murmur, which is not conducted over the usual aortic area, nor toward the apex around the chest. The second sound is decidedly roughened. The pulse is now 30. At 4 P. M. the pulse was 46, steady and regular. Cannot pass urine in the horizontal position. Bladder emptied with a catheter.

6th (eighteenth day). Condition improved. Pulse 58; steady and regular. The murmur is unchanged. Pain and swelling in the right wrist. The patient is nervous and hysterical. Slight nocturnal delirium. The evening temperature is 102.4°.

7th (nineteenth day). Had a very quiet night; feels comfortable and refreshed. Pulse 70, but intermittent in about one beat in five. Condition of heart unchanged. Pain has left entirely the right wrist, and gone to the left. Passed water freely without the aid of a catheter. 5 P. M., pulse intermits at every eighth beat. Murmur unaltered. The pain is now centred in the calf of one leg. Is passing a considerable quantity of watery urine. At 8.30 the temperature was 103°.

8th (twentieth day). 10 A. M., pulse 90, temp. 101°, resp. 20. Passed a restless night, owing to the pain. Both wrists are now very painful, and slightly swollen. The pulse is regular; no intermissions. The murmur has disappeared. Is unable to pass water unaided.

10th (twenty-second day). Pain is now in the left foot, the wrists being quite free. Heart murmur is again audible, though it is by no means as loud as formerly. Urine is passed naturally.

June 1. There was gradual improvement up to the last week in May. The heart symptoms had disappeared, and there was no more pain in the joints. About the 28th of May, the temperature began to range high, reaching 100.4° for three consecutive nights. There was also frequent pain in micturition, the urine being loaded with lithates and mucus; no albumen or sugar. On May 29th, there were decided pain and tenderness in the perineum, and on the 30th a small abscess burst into the lower part of the rectum, after which he felt much relieved.

The case progressed favorably, though slowly, to

complete recovery, the patient being now (August 1) able to resume his ordinary pursuits.

Remarks.—The interest of this case centres in the remarkable slowing of the pulse, without which, it would be regarded simply as a case of acute rheumatism with a heart murmur, disappearing on the recovery of the patient.

Some doubt might, however, be thrown upon the original diagnosis of rheumatism, but what other disease could produce a lengthy illness such as this, with metastatic pains in the joints, with profuse sweats, and heart complication? None with this exact combination of symptoms; but, after the event, looking back over the history, might not all be accounted for by the suggestion of septicæmia? The actual occurrence of suppuration lends force to this hypothesis. The presence of pus hidden away in the ischio-rectal fossa, may have caused the symptoms from the first. Certain it is, that they improved as soon as the abscess was emptied. However, none of the more pronounced symptoms of blood poisoning were present. There were no rigors, no irregular pyrexia, no diarrhoea, no permanent joint swelling, no oscillations of the temperature, and, after all, the small abscess was, most probably, of but a few days' formation.

The cardiac complication, I regard as depending upon the action of the rheumatic poison upon the innervation of the heart, and not upon the presence of endocarditis. All considerations point to the functional character of the murmur. Records exist of the slowing of the pulse under nervous influences, to a point as low as fourteen or even twelve, though I am unable to find a case in which such an occurrence was noted in connection with acute rheumatism. The character of the murmur was not such as might be expected to occur in ordinary rheumatic endocarditis. It was not distinctly aortic, decidedly not mitral. It was heard over the right ventricle extending from the sternum to the nipple. It seems probable that it was a tricuspid murmur, either the result of passing endocarditis or the result of disordered cardiac innervation, the latter seeming to me to be the more reasonable explanation. Lastly, the symptoms could not have been caused by any remedy administered at the time, for during the forty-eight hours preceding the attack, he had had no internal medicine at all, the colchicum having been stopped. A liniment of camphor, aconite, and belladonna was in use, but it had been applied for several weeks previously, without producing any untoward symptoms.

MEDICAL PROGRESS.

THE HYGIENE OF THE NEWLY BORN.—The following instructions to mothers and nurses, prepared by a commission composed of Moutard Martin, Bergeron, Parrot, Blachez, and Dujardin-Beaumetz, have been issued by the head of the Department of Public Charities, Paris:

1. Till the appearance of the first teeth—*i. e.*, between the sixth and seventh months—the only food of the infant should be milk, that of the mother preferably, if she be in good condition, otherwise that of a wetnurse.

It is very dangerous to give an infant solid food of any kind during the first months of its life.

2. The child should be offered the breast about once in two hours, and less often in the night.

3. In the event of inability to provide woman's milk the milk of the cow or goat may be substituted. This milk should be given warm, diluted with one-fourth part water, and slightly sweetened. At the beginning of the fifth month the milk may be taken pure. All other liquids employed to dilute the milk (thin gruel, bread water, barley water) are injurious.

4. In feeding the infant, glass nursing bottles should be employed. These, especially the tubing and the mouth piece, should be thoroughly cleansed every time they are used. Never allow nurses to resort to those "sugar teats" with which some mothers seek to appease the cries of the infant, and which are sure to produce canker, and disorder the digestion.

5. It is not till the sixth or seventh month that one should begin to allow farinaceous substances with milk, such as bread, baked flour, rice, arrowroot, mealy potatoes; these supplementary foods should always form a considerable part of the infant's dietary toward the end of the first year, to accustom the child to weaning. Weaning ought not to be thought of till the first twelve or sixteen teeth have pierced the gums, while the infant is in a good state of health, and during the lull which follows an eruption of teeth.

6. Every morning the "toilet" of the little one should be made before suckling or feeding; this toilet consists: (1) in washing the child's body, and especially the genitals, which ought always to be kept clean; (2) in scrubbing the head, on which it will not do to let scurf or dandruff accumulate; (3) in changing (at least every second day) the child's underclothing; (4) in giving a warm bath, in which the infant should be held five or six minutes. The belly band ought to be kept on during the first month.

7. Swaddling clothes, which cause compression of the body, should be interdicted. The more freedom the young child has in its movements, the more robust it becomes, and the better its development. All swathing which encumbers the neck and head should also be discarded.

8. The infant should be protected against the injurious effects of excess of cold and heat, whether outdoors or in the house; within doors, the air should be renewed several times a day.

9. It is not safe to carry the babe into the open air before the fifteenth day, unless the temperature is very mild.

10. The child ought not to be allowed to sleep in the same bed with its mother or nurse.

11. The bed of the infant should be composed of oaten straw, soft thatch, or husks; the cradle should have curtains during the first months of infancy, and especially during the cold season, to avoid currents of air, but these curtains should never be completely closed. The babe ought not to be rocked.

12. There should not be undue haste in teaching the infant to walk; it should be allowed to creep on the floor and help itself up; walking carts and baskets should be discountenanced.

13. The least indisposition on the part of the infant (colic, diarrhoea, vomiting, cough) should be at once attended to.

14. As pregnancy has the effect of rendering the milk less nutritious, in case of pregnancy every nursing woman should cease to suckle her infant.

15. It is a good plan to vaccinate infants during the first three months after birth, or during the first few weeks, if an epidemic of smallpox is prevailing.—*Boston Medical and Surgical Journal*, April 8, 1886.

TWO CASES OF BRONCHIECTASIS TREATED BY PARACENTESIS.—DRS. WILLIAMS and GODLEE presented before the Royal Medical and Chirurgical Society the following report: The first case was that of a gentleman, aged sixty-seven, who had suffered for two years from chronic bronchitis and emphysema, and six months later had dry pleurisy of the right lung. After the pleurisy, the expectoration, before moderate, became profuse and somewhat fetid, the cough more and more harassing; and, although Malvern and Bournemouth were visited, and a great amount of antiseptic treatment was tried, he obtained no relief. On February 3d he was seen by Dr. Williams, who noted the existence of a bronchiectasis in the lower lobe of the left lung, the pleura of which was adherent. Tubular sounds were heard over two small areas, neither exceeding a half-crown in circumference, in the eighth interspace. At the request of Dr. Williams, a puncture was made by Mr. Godlee into the first area, with an aspirating trocar and canula. Under antiseptic precautions, the cavity was opened, and a drainage tube inserted. The result of the operation was, that the cough nearly ceased; the expectoration, which was for a few days pneumonic, diminished from one pint a day to a few pellets, and became free from odor; and the patient gained flesh rapidly. At the end of six weeks, the discharge ceased, the channel closed, and the wound healed up. Examination of the chest showed complete disappearance of tubular sound from the second area, as well as from the first, and further shrinking of the chest-wall. The patient, at the close of nine months after the operation, remained free from cough and expectoration, and was able to walk four miles at a stretch. The second case was that of a girl aged twenty-one, who, ever since an attack of typhoid fever, had had troublesome cough and copious expectoration, gradually increasing in quantity, and becoming more and more fetid. She had had hæmoptysis three times, on one occasion amounting to a pint. Examination of the chest showed several bronchiectases, two of which gave rise to very coarse râles over areas about the size of a half-crown in the sixth and seventh interspaces in the axilla; a third area was detected in the eighth space below the scapular angle. At first, the patient improved under antiseptic treatment, the expectoration diminishing and becoming less fetid; but after a while these measures ceased to give relief, and an operation was decided on. On June 29th, at Dr. Williams's request, Mr. Godlee passed a small exploratory trocar successively into each of the three marked spots, and, obtaining no result, inserted a large sized aspirating trocar and canula into the seat of the first puncture in the sixth interspace. But an attempt to cut down on the cavity failed. A second operation was performed on July 16th, and, after some exploratory punctures, Mr. Godlee, with antiseptic precautions, cut down on and laid bare the eighth rib, and excised about an inch of it in order to approach nearer to the bronchi

before attempting to open them. The trocar and canula were then passed to a depth of five inches, and Mr. Godlee cut down along the canula, and inserted a drainage tube. The wound gradually healed, and the patient left the hospital at the beginning of September considerably improved, the cough slight, the sputum having diminished to about three ounces a day, and being only occasionally fetid. The presence of bronchiectases in other parts of the left lung, and possibly also in the right lung, precluded such complete success as was obtained in Case 1. A short account was then given of four other cases of bronchiectases treated by tapping, in all of which the position of the cavities had been recognized and successfully punctured, but, owing to the presence of other bronchial dilatations, the success was only partial in these cases. The arguments in favor of the operation were: 1. The tendency to death by septicæmia in some form, unless proper drainage was effected; 2. The reduction in the amount of expectoration, this being due, not only to the removal of the matter, but to the disappearance of the effects of its irritation on the healthy bronchi; 3. The invulnerability of the lung tissue; it being proved that punctures of this tissue gave rise to little or no disturbance to the part, or to the system generally. The difficulties of the operation were principally those of diagnosis of the exact positions of the bronchiectases: (1) from the presence of emphysema; (2) from the reverberatory character of their auscultatory sounds, which rendered exact localization very difficult. Paracentesis of bronchiectases seemed to be indicated under the following circumstances: 1. In cases where, antiseptic treatment of all kinds having failed to correct the fetor of expectoration, and to allay the harassing nature of the cough, death by septic pneumonia seemed imminent. 2. Where the evidence went to prove that the bronchiectases were confined to one lung, were situated in the lower lobe, and had overlying them an adherent pleura. It was not indicated where multiple bronchiectases existed in both lungs, where they were surrounded by emphysema, and where the pleura was non-adherent.—*British Medical Journal*, March 27, 1886.

CHRONIC TEA POISONING.—As a result of the analysis of seventy-four cases of chronic tea intoxication DR. WILLIAM N. BULLARD, of Boston, records the following conclusions:

1. That the action of tea is cumulative.
2. That its action is more pronounced on the young and on those subject to anæmia or in a depressed physical condition, although persons otherwise healthy not infrequently show toxic symptoms.
3. That among the class of people under consideration, who, as a rule, use medium grades of Oolong and English Breakfast tea the average amount needed to cause toxic symptoms is a little less than five cups per diem.
4. That chronic tea poisoning is a frequent affection, and that its most common symptoms are loss of appetite, dyspepsia, palpitation, headache, vomiting, and nausea, combined with nervousness and various forms of functional nervous affections, hysterical or neuralgic. These symptoms are frequently accompanied by constipation and pain in the left side or cardiac region.—*Boston Medical and Surgical Journal*, April 8, 1886.

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ON REDUCTION OF UTERINE INVERSION.

UTERINE inversion, although of infrequent occurrence, is of such grave import that any contribution to our knowledge of the subject demands careful consideration. DR. JAMES H. AVELING, in a recent interesting lecture upon inversion of the uterus, in the *British Medical Journal* of March 13th, gives three modes of inversion, fundal, lateral, and cervical. In the first, the fundus of the uterus first passes through the os; in the second, one of the sides of the uterus; and, in the third, the process begins in the lower part of the uterus. Theoretically, this classification may be scientific, but practically it is of no value, as in a complete inversion it would be impossible to know where the process began; and so, too, were this known, we would not be in the least assisted by the information in effecting the reduction.

In describing the causes of inversion, the author gives one form of the accident as automatic, and states that it results from the contractions of the uterus. "It is always of the fundal variety, and is determined by an indentation of the top of the uterus, or by some body attached inside to the fundus." But this cupping, or indentation of the uterine fundus, is not automatic, while the subsequent contractions, which complete the inversion by acting upon the introcedent fundus, are automatic. In the production of this variety of the displacement importance is given to the attachment of the placenta to the fundus; and so, too, in lateral inversion, like importance is given to its attachment to one of the sides of the uterus; but we must remember that the placenta is very rarely attached to the fundus, still more rarely to the side of the uterus; and, therefore, it is quite possible that some of the cases both

of fundal as well as of lateral inversion, when caused either by the spontaneous dragging of the placenta, or pulling upon the cord, were really instances of inversion beginning in the posterior or anterior uterine wall.

In giving the diagnosis of recent inversion, Dr. Aveling makes the following statements: "Manual examination discovers a large, rounded mass in the vagina, which can be pushed up, replaced, and does not fall again, as would be the case with all the other presenting bodies for which it might be mistaken. The fingers passed through the os are also arrested, and no uterine cavity is to be found. The sensibility of the inverted uterus is another valuable diagnostic symptom." Now, we believe there are some important errors in these statements. Possibly, if an inverted uterus be examined immediately after the accident, "the large, rounded mass" described will be found; we have never had an opportunity of making such an examination; but if the examination be made forty-eight hours after the accident, the uterus may be found pyriform, and very little larger than an inversion of some weeks' duration. Then, as to this mass being "pushed up, replaced," the words, if they mean anything, simply indicate that the dislocation is reduced, and there is an end to any question of diagnosis. The third error which we wish to point out, is that "the sensibility of the inverted uterus is a valuable diagnostic symptom." While theoretically this statement is true, yet we know no observed facts which prove it; on the other hand, we do know that the inverted uterus may be pressed by the grasping hand, and may have an exploring needle thrust into its tissue without the patient complaining; the introduction of the hand into the vagina will cause pain, but not the pressure upon the uterus, or its puncture. Now let us imagine a case in which a pyriform tumor, such as has been described, is found in the vagina forty-eight hours after labor, and this tumor is insensitive. Abdominal examination discovers a roundish, resisting mass which reaches to the umbilicus, pressure upon which causes escape of clots from the vagina; and if one goes no further in his examination—if he accepts Dr. Aveling's statements as to the form and as to the sensibility of the inverted uterus—he will surely be led into a dangerous error. But if, notwithstanding the statements of nurse and professional attendant that the patient urinates freely, he pass a catheter into the bladder, he may find that the abdominal tumor is caused by the distended bladder. In every doubtful case, then, we insist upon this matter as one of the highest importance: a catheter should be introduced into the bladder, so as to be sure that this organ is empty. Error will be thus avoided, and the possibility of bimanual examination secured.

In referring to methods of reduction of chronic

inversion, that is inversion continuing after involution has been completed, Dr. Aveling speaks of White's method as "a combination of taxis with violent pressure, by means of a ten-pound spring against the chest." This statement does scant justice to a method which counts so many successes, so few failures. The "violent pressure" exists only in the minds of our British brothers, and the method includes counter-pressure, and really is as little likely to do harm as "the sustained elastic pressure" recommended by Aveling, in which the force exerted cannot be certainly known. The first use of elastic pressure by means of rubber bands is credited to Dr. Robert Barnes. Barnes used a pelvic-curved repositr, Duncan a straight one, while Aveling's has a sigmoid curve, and hence his method may be called a modification of Duncan's modification of Barnes's method. Aveling's repositr has a vaginal and a perineal curve, so that theoretically the pressure is made "in the direction of the axis of the uterus." Ten cases are narrated in which the instrument was used successfully for the reduction of chronic uterine inversion; the longest period required for reducing the dislocation was fifty-four hours and a half, the shortest nine hours. A case, however, is quoted from Atthill in which the use of the instrument failed.

It is probable that Dr. Aveling too greatly exalts his method, not recognizing the value of any other, as he certainly is in error in the assertion that "every case of chronic inversion of the uterus can be cured by sustained elastic pressure exercised in the right direction." The possible adhesions between the peritoneal surfaces of an inverted uterus, as demonstrated in a case reported by Velpeau, would certainly be an effectual barrier against reduction of the inversion, even though the sigmoid repositr of Aveling were used.

Aveling quotes Newnham, who wrote in 1818, as saying that "it is wisest and best to reinvert the uterus by returning that portion of it which was last expelled from the os uteri." Obviously, in adopting this rule as one to be followed in the reduction of chronic inversion, Aveling forgets the cases where the inversion began at the cervix. Again, he credits Newnham with the enunciation of a rule, in 1818, which was just as plainly stated by Leroux, in 1776, and had still earlier been suggested by Astruc and by Puzos.

Notwithstanding Aveling's strong recommendation of his method of reducing chronic inversion of the uterus, we certainly prefer that of White, which is accomplished in a shorter time, and in which the operator assists the action of the repositr by counter-pressure, and by stretching, with his finger in the vagina, the collar-like projection of the cervix, thus facilitating directly the restoration of the lower part of the inverted organ first. We would reserve

continued elastic pressure for such cases of this dislocation in which the use of taxis and a repositr has been in vain.

THE BLOOD PLAQUE.

THE chief interest of PROFESSOR OSLER's Cartwright Lectures, the last of which appears in this week's issue of *THE NEWS*, lies in the full consideration of the much debated third corpuscle for which the term plaque is suggested. This element has had a hard struggle for recognition at the hands of histologists, and even yet there are capable observers who are not convinced of its existence. The difficulty lies in the remarkable rapidity with which the corpuscles undergo alteration when the blood is withdrawn, fusing into irregular masses in which the individual elements lose their distinctness. In order to see them clearly, they must be studied within the vessels, or in the blood-drop expressed directly into osmic acid or some fluid which will prevent their adhesion to each other. Under these circumstances it is easy to determine the presence of the plaques, and the conditions are such as to render it almost impossible that they should arise from the disintegration of the other corpuscles. The balance of testimony is strongly in favor of the views of Osler, Hayem, Bizzozero, that they are preëxistent, independent blood elements.

Their origin does not appear to have been determined, but their relation to the development of the red corpuscles, and to the process of coagulation, are questions which have aroused very considerable interest. Hayem gave the term *hæmatoblast* to the corpuscle, believing that it represented the early condition of the red corpuscle, of which the microcyte was an intermediate stage. The discoid shape, the gradations in size, the abundance in the blood in the young, and in the adult in conditions when blood-making is active, favor this view; but other observers have not been able to detect the gradual tinting of the plaques, and the intermediate forms which Hayem describes, and their relation to the regeneration of the corpuscles remains doubtful.

An important part of the work of the past few years on these bodies relates to their connection with coagulation and thrombosis, and the facts which are given in the third lecture, if corroborated, will necessitate a modification of the current views of the physiology of these processes. From the first, Hayem insisted that the *hæmatoblast* played an important rôle in coagulation, but his observations did not arouse the attention which they deserved. Since the issue, however, of Bizzozero's paper, in 1882, the subject has been very carefully studied, and the evidence has gradually accumulated in favor of the view that these bodies share, at any rate, with the colorless corpuscles in the formation of fibrin. That

the leucocytes undergo disintegration as coagulation proceeds, would appear to be established by the experiments of Schmidt's pupils and of Wooldridge, and yet we are told in the study of the histogenesis of fibrin with the microscope, under conditions the most favorable for the detection of any changes in the colorless corpuscles, it does not seem possible to demonstrate their participation in the process. There is a glaring contradiction here which subsequent observations must explain. The evidence brought forward by those who maintain that the plaques are important agents in coagulation, may be thus summarized: First, they are the elements which immediately adhere to any foreign body within the vessel, or to its cut edges, if wounded; second, in circulating blood the plaques may be shown to be the bodies which aggregate upon any laceration and form the basis of the thrombi so produced; and third, they compose the structures known as white thrombi.

It is to be hoped that the presentation of this subject will stimulate further research, and enable us before long to pronounce more definitely on the relation of these elements to blood formation.

THE ASSIMILATION OF IRON.

MUCH difference of opinion has existed as to the method of action of ferruginous tonics. That their use is of advantage is a matter of daily observation, but many difficulties arise when we attempt to explain their mode of assimilation, for, apart from the fact that nearly if not quite all the iron so ingested is recoverable in the feces, we are met with the equally perplexing fact that iron salts when introduced into the blood stream cause toxic symptoms analogous to those induced by arsenic.

It has long been recognized that the iron entering into our structure is not normally derived from any inorganic salt, but from one or more complex iron-containing compounds existing in our food, and to be found typically, of course, in milk. BUNGE, in the *Zeitschrift für physiologische Chemie* for 1885, records the extraction, from milk and from egg yolk, of this iron-containing organic compound, to which he gives the name of hæmatogen.

Hæmatogen markedly resembles hæmoglobin in molecular composition, though a still more close molecular resemblance may be traced between it and nuclein, if we ignore the absence of iron in the latter body. Bunge has extracted hæmatogen from the cereals and leguminosæ, and states very distinctly that our food "contains no inorganic iron combination, the iron present being in the form of complex organic compounds, which are built up by the vital activity of the plant; that in these forms the iron is absorbed and assimilated; and that from them the hæmoglobin originates."

Starting from these premises, Bunge's explanation

of the value of inorganic iron salts in chlorosis is very interesting. The catarrhal state of the alimentary tract present in this condition favors a process of fermentation which induces the decomposition of hæmatogen. But when the inorganic iron salts are present, the sulphites evolved in decomposition attack such salts, with the result of sparing the hæmatogen. Confirmatory to this theory is the recent method of treatment of chlorosis, in which the disinfection of the digestive tract by the administration of small antiseptic doses of hydrochloric acid, after meals, has been found more efficient than the use of iron.

FILARIA SANGUINIS HOMINIS.

THE discovery by DR. GUITÉRAS that this hæmatozoön is native in the United States, as published in *THE MEDICAL NEWS* of last week, is an exceedingly interesting contribution to helminthology. Heretofore, the cases which have been observed in this country have been uniformly imported, and at Key West Dr. Guitéras has had four examples in Cubans. The Charleston patient is a female, the subject of chyluria, a condition most frequently associated with the presence of a parasite, and from the history there can be no question that she contracted the disease in one of the Southern States, either Georgia or South Carolina. In a second case, a male with commencing elephantiasis of the scrotum, the patient has never been outside of Charleston. It is highly probable that a considerable number of the cases of chyluria and elephantiasis occurring in the Southern States will be found associated with the filaria.

These hæmatozoa are widely distributed throughout the animal kingdom, and they may occur in enormous numbers without causing apparent inconvenience. In the dog, in the country, they are not at all uncommon, particularly in the South, and in the Wister and Horner Museum of the University of Pennsylvania may be seen the specimen presented by Dr. Joseph Jones, in which the right heart is full of the adult parasites. It was from this specimen that Dr. Leidy gave his original description of *filaria immitis*.

Practitioners will do well to remember that there are cases of chyluria and hæmato-chyluria of non-parasitic origin, and care must be exercised in the examination of the blood, the specimen being taken during the night, at which period alone the embryos are to be found in the blood.

THE DETERMINING CAUSE OF LABOR.

THE determining cause of labor has been the subject of discussion for hundreds of years, and we are as ignorant now as they were who began the discussion. Avicenna was content to say that labor came on at the end of nine months by the grace of God,

and to-day we can only state that labor occurs at a definite time according to a law of nature.

One of the many improbable hypotheses which have been suggested as the determining causes of labor, was that of the late DR. TYLER SMITH; he held that the *primum mobile* was in the action of the ovaries. We have often wondered why some of those who are engaged in vivisection did not try removal of the ovaries in some of the inferior animals while they were pregnant; were this done, and the hypothesis of Smith be true, these animals would have their pregnancies indefinitely prolonged, for there would not be for them any ovarian nixus, or impulse, to start the machinery of parturition. Such experiments, we believe, have never been made, although it is some years since they were publicly suggested. Meantime, however, the experiment has been made upon the human female. In England a pregnant woman had both ovaries removed, and she recovered perfectly from the operation, but, with total disregard for Tyler Smith's explanation of the etiology of labor, she got rid of her offspring in the natural way, if not quite at the normal time. Of course, one case does not disprove the hypothesis, but it adds to its generally conceded improbability. One might indeed suggest that, possibly, in this case the removal of the ovaries was incomplete, that some fragment of ovarian tissue was left, or that there was a supernumerary ovary, but a hypothesis which rests upon hypothesis has a very insecure foundation.

SOCIETY PROCEEDINGS.

NEW YORK ACADEMY OF MEDICINE.

Stated Meeting, April 1, 1886.

THE PRESIDENT, A. JACOBI, M.D., IN THE CHAIR.

DR. HENRY C. COE read a paper entitled

IS DISEASE OF THE UTERINE APPENDAGES AS FREQUENT AS IT HAS BEEN REPRESENTED?

In commencing his essay he stated that it was not intended as defiant scepticism, but as a mild negative. There is much yet to be learned in regard to the pathology of the uterine appendages. While in all modern systematic treatises on the diseases peculiar to women, many chapters are devoted to ovarian tumors and their removal by surgical procedure, very little is to be found in regard to ovaritis and peri-ovaritis, and that little, wholly unsatisfactory.

Agnosticism in regard to ovarian and tubal disease meets with very little encouragement at the present day, and this is due to a large extent to the surgeon. Having put the query, what constitutes a normal ovary? Dr. Coe said that this was a thing which has come to be somewhat problematical, except in the case of some of the lower animals. In considering the condition and appearance of the ovary in any given case, it must not be forgotten that during the entire sexual life of the female constant changes are going on in these organs.

Hence, it is very difficult to say where the normal ends and the pathological begins. Nothing is now more frequent than to remove ovaries because they are "cystic," on the ground that, in consequence of this condition, they are functionally useless. Yet Dr. Coe has seen cysts as large as a marble which could not be called pathological, because they contained perfectly healthy ova which were certainly capable of being impregnated.

Taking up the pathological conditions of the ovary, as viewed by the laparotomist, he said that many of the variations of the organ, in size and shape, which were commonly regarded as pathological by him, were found in ovaries which were to be regarded as, in fact, altogether normal, at least, so far as function was concerned. Congestion and moderate enlargement do not necessarily imply chronic oöphoritis, and indicate oöphorectomy; since, when the other pelvic organs are for any reason in a condition of hyperæmia, the ovaries are naturally found hyperæmic also.

In the same way a decided diminution in size may not be pathological, but merely an indication of senile atrophy. Again, certain conditions which are regarded by the laparotomist as pathological, such as thickening of the external covering of the organ, may not be so in reality, since such thickening is not at all abnormal in senile atrophy, or after very frequent ovulation, when localized thickening is very common, even when senile atrophy does not yet exist. But, although there may be exterior thickening, it is no indication that there is cirrhosis of the internal stroma of the organ which interferes with its functions.

The most frequent causes for which ovaries are extirpated, however, is so-called cystic degeneration, and Dr. Coe remarked in this connection that if the slightly cystic condition which is so frequently met with in the ovary is of grave import, few women are free from the danger of serious cystoma.

He next passed on to consider the subject from a microscopical point of view. In order that the pathologist may give an intelligent opinion concerning any removed ovary presented to him for examination, he must be familiar with the normal histology of the organ, and, on account of the constant changes going on in the ovaries, previously referred to, this is a matter which involves no little difficulty. The pathological appearances of well-marked disease are easily recognized; but the lesser ones must be acknowledged to be very difficult to differentiate. Again, because one portion of the organ is diseased, it is no reason why the whole of it should be involved in the pathological process; and, acting on this principle, Schröder has lately been operating in such a way as to remove the diseased portions and allow the rest to remain. The number of cases thus far recorded, however, is too small to base any positive conclusions upon as to the practical utility of this method. At present, this seems somewhat problematical; but it is, at all events, gratifying to note it as a step in the right direction.

Tubal Disease.—During the three years that have elapsed since Tait called special attention to this condition, and Thomas popularized Tait's operation in America by his paper before the Academy of Medicine, this procedure has been resorted to an immense number of times; and there can be no doubt that it has been

greatly abused. No reasonable objection can be raised to the removal of Fallopian tubes which are filled with pus; but this is not the case with a large number of the tubes that are taken out.

Tait says, that in chronic disease of the ovaries the tubes are always affected; but Dr. Coe denies this, as the result of his personal pathological observations. Some of the so-called diseases of the tubes are not recognizable by the pathologist at all. There is no doubt about pyosalpinx or hydrosalpinx, when these conditions are present; but as to so-called catarrhal salpingitis, "commencing pyosalpinx," and other presumed pathological conditions which constantly pass unchallenged in learned societies, there is very good reason for doubt. In cases where the diagnosis of catarrhal salpingitis has been made, and the tubes and ovaries removed in consequence of the condition, Dr. Coe has found, on subsequent examination, simply moderate hyperæmia, with a thin coating of mucus, which may exist in a perfectly normal Fallopian tube; there being no evidence of inflammation whatsoever. So long as the lumen is free and the lining mucous membrane capable of performing its function, the tube is not abnormal; notwithstanding the fact that there may be slight adhesions of the fimbriated extremity.

From his examinations he would say that pyosalpinx exists in about one-fifth of the cases where Tait's operation is performed. Hydrosalpinx is considerably less common, and hæmatosalpinx is extremely rare. If there is no pus discoverable, there is certainly no pyosalpinx; just as in general practice we say in a case of suspected abscess, that if there is no evidence of pus, there is no abscess.

The pain that is so often attributed to disease of the tubes, Dr. Coe believes to be generally due to localized peritonitis or to neuralgia pure and simple. There can be no doubt that far too many tubes and ovaries are removed at the present day, and the one criticism that he would make on American laparotomists is, that they do not follow up their cases for a sufficiently long time. As a rule, they are reported very shortly after the operation, when the results often seem all that could be desired; but if the histories could be given of the condition from six months to two years later, it would not infrequently be found that no permanent improvement whatever had resulted from the procedure. He has met with many women under these circumstances who still have the same pain as before the operation.

The deductions drawn by Dr. Coe are somewhat as follows:

(1) Ovarian disease is not as common as it is now generally supposed to be. This impression is derived from the surgeon, and not the pathologist.

(2) Because the ovary is partially diseased, it does not follow that the whole organ is implicated.

(3) "Cystic" and "cystic degeneration" are mischievous terms, which have been productive of much evil by leading to false conclusions, and unnecessary ovariectomies in consequence.

(4) Actual disease of the tubes is far less frequent than many surgeons would have us believe.

(5) Localized peritonitis will account for much of the trouble attributed to disease of the uterine appendages.

Dr. Coe said, in conclusion, that, while the normal histology of these organs is not well understood, and

their various pathological conditions must, therefore, necessarily be regarded as still *sub judice*, he was heartily in favor of the performance of laparotomy in all perfectly legitimate cases.

DR. LUSK said that the author of the paper had covered nearly all the ground which he had himself been accustomed to take in the tirades against the abuse of Tait's operation, with which many of his gynecological friends are now familiar. He was by no means an enemy of the operation, and thought it did great good in its place; but he had to say that he had stood by and seen tubes removed which are described by various pathological names, but which seemed to him entirely normal. In the same way he had seen ovaries taken out, of which it was said that they were in a state of commencing cystic degeneration, but which appeared to him typical examples of physiological ovaries, precisely like those he had formerly been accustomed to exhibit to his class as such when he used to teach the branch of physiology. He then took great interest, he said, in pointing out the changes which are constantly going on in these organs; and these so-called cystic ovaries, which had been removed for merely imaginary disease, were simply ovaries which were undergoing these same physiological changes.

Since surgeons had come to be able to remove the ovaries and tubes with very little risk to the patient, the operation, he believed, was performed entirely too frequently. Tait's advice is to cut open the abdomen and examine the condition of the ovaries and tubes; when, if they are found all right, they are to be returned to their places in the pelvic cavity. But he himself had yet to meet with the surgeon who after getting thus far in the operation, did not take out the organs altogether, whatever their condition might be.

He believed that there are very few cases in which the condition of pyosalpinx cannot be diagnosed by proper manipulation with the fingers of one hand in the vagina, and that the great mistake of the present day is the too frequent opening of the abdomen. Unless the enlarged tube can be felt, in the manner described, outside the body, the operation ought not to be performed; and unless this rule is adopted, the temptation is to take out too many tubes.

He then went on to say, that it seemed probable to him that in the future a modification of Tait's operation would come into vogue, which would obviate the necessity of removal of the tubes even in pyosalpinx and hydrosalpinx. The time would come, he thought, when these distended tubes would be evacuated through the vagina by means of some such instrument as Dr. Mundé had devised, which consists of a syringe with a hypodermic needle attached to it. In this connection, he related a case of pyosalpinx which had recently come under his observation. He had secured the pavilion at the hospital for the patient, engaged the nurses, appointed the day for the performance of laparotomy, and invited a number of friends to be present at the operation; when, a short time before the time fixed for the latter, the woman noticed an unusual discharge from the rectum, and in a day or two the tumor caused by the distended tube had completely disappeared.

As to the removal of the ovaries and tubes, when these organs are normal, for the relief of neuralgia and

other conditions, the result is very uncertain, and the disturbance of the general system by the operation is so great that the most serious consequences are liable to ensue. In one case of his own, in which he had been obliged to remove the ovaries and tubes in connection with a large fibroid which demanded extirpation, the patient had been rendered insane by the operation.

Dr. W. GILL WYLIE said that he did not propose to defend any one for taking out normal ovaries. He did not see how the question under consideration could be settled, if an absolute distinction be drawn, as seemed to be done by Dr. Coe, between the surgeon and the pathologist. In the cases in which he had operated, his ground for doing so had been the symptoms of the patient and what he could detect before opening the abdomen. He had always been especially careful as to diagnosis, so that he could be able to defend his position when the reaction in opinion in regard to Tait's operation, which he constantly expected, had set in. While at first he did not feel so sure about the matter, he could now, after etherizing the patient, almost always tell exactly what condition he was going to find upon performing laparotomy. At the present time he very rarely does the operation except for pyosalpinx.

Unless the *symptoms* justify it, he does not operate, whatever the condition that is indicated by local examination. There are many cases of pyosalpinx in which the enlarged tube falls behind the round ligament, and there is very little danger of its bursting into the peritoneal cavity. It is a nice point in diagnosis to distinguish those cases that are likely to burst and give rise to peritonitis; and in instances where there have already been several attacks of localized peritonitis, this result is to be apprehended. When, however, the diseased tube is folded under the broad ligament, the pus, as a rule, points below.

Dr. Wylie went on to say that he operated to cure his cases, and in order to produce a cure, there is no operation that can compare with laparotomy. Aspiration merely evacuates the pus, which is sure to return, and this procedure may cause fatal injury by wounding the ureters, etc. As to the matter of peritonitis, he believed that the vast majority of cases, perhaps ninety-nine out of one hundred, of local peritonitis are associated with disease of the ovaries and tubes.

There were two doubtful cases out of the twenty-two in which he performed laparotomy last year. In these the operation was for the relief of hystero-epilepsy.

Dr. WM. M. POLK said that some remarks which he made before the New York Obstetrical Society about a year ago, contained the gist of the solution of this whole subject. Dr. Lusk had remarked that in the ease with which the operation is now performed lies the great danger of its abuse. But at the same time he would like to add this fact, that salpingitis is by no means a new disease. It is a pathological condition which has always existed, and which, moreover, has been cured in a large number of cases without laparotomy.

These are cases which used to be treated, in accordance with the suggestion of Emmet and Sims, by putting the patient to bed, using opium freely, and poulticing the abdomen. The women got well, with sound ovaries and tubes, and afterward had children. In a case of this so-called pelvic cellulitis (as it

was formerly considered), he asked a distinguished New York gynecologist to make an examination. He did so, and pronounced it to be undoubtedly one of cellulitis. Dr. Polk insisted that he was mistaken, and said that he would prove it if the gentleman would come to the laparotomy which he proposed to perform in the case in three weeks' time. Accordingly, he was present on that occasion. There were induration on one side, and a laceration of the cervix involving the vaginal roof. Dr. Polk cut open the abdomen, and it was found, on examination, that there was a cicatricial thickening in the roof of the vagina; but in between the layers of the broad ligament there was no induration whatever.

In fifteen cases in which he would have made the diagnosis of cellulitis, according to the views in which he was educated, he had performed laparotomy and taken off from the posterior fold of the broad ligament a conglomeration of ovaries, tubes, and new tissue; but, as in the instance just mentioned, there was no induration at all between the layers of the broad ligament, although on external manipulation there had been found a hard mass or tumor in the neighborhood of the broad ligament. He believed, therefore, that the cases now recognized as pyosalpingitis, are those formerly called pelvic cellulitis, and often successfully treated by confinement to bed, opium, poulticing, the douche, etc. As a rule, such cases usually get well; but sometimes they terminate fatally.

The peritonitis which he was accustomed to find he thought came through the tubes. Dr. Coe had said that disease of the tubes is rare; but he must confess that he met with it very often. They did not always contain pus, but, at all events, they presented the evidences of furious inflammation. From his experience in connection with this subject, he has reached the conclusion that localized peritonitis is usually produced from extension of inflammation from the interior of the uterus. Inflammation prefers to take the course which is open to it. In a large number of cases of so-called cellulitis, the trouble is preceded by ordinary endometritis, which may be produced in a thousand ways. This leads directly to inflammation of the tubes, and, finally, of the peritoneal cavity. A simple catarrhal inflammation can as easily extend to the peritoneum as can the poison of gonorrhoea. He would not say that the inflammation might not in certain instances follow the course of the lymphatics and bloodvessels, but this he believed to be a rare exception.

The purulent collections which have heretofore been supposed to be in the folds of the broad ligament are to be regarded, except when they are the result of bad puerperal inflammation, as nothing more than the accumulation of pus in the tubes, which, if the quantity increases rapidly, is likely to escape into the adjoining tissues. The fimbriated extremities of the tubes usually become attached to the broad ligament.

The essential point of this whole matter is that we must change our pathological views before we can reach a unanimous opinion in regard to the advisability of operation. There are a small number of cases which require to be operated on. They are the ones in which there is localized peritonitis with salpingitis, and usually an atrophied ovary.

Dr. P. F. MUNDE said that his clinical experience with affections of appendages of the uterus had been

large, and he had taken the trouble in connection with the subject of this evening's paper to look up some statistics regarding it. Out of 3000 cases treated by him in private and hospital practice, 117 were put down as oöphoritis and salpingitis combined with peritonitis.

This showed agreement with the views expressed by Drs. Polk and Wylie, and in his experience he had found that these three conditions almost always coexist.

Besides, there were noted 212 cases of simple, non-complicated oöphoritis. These figures, he thought, answered the question of the title of Dr. Coe's paper. He would say, therefore, that affections of the uterine appendages are quite as frequent as Tait and his followers assert that they are. He would, however, by no means say that the operation of laparotomy for their relief is required as often as they claim. Dr. Mundé said that he did not agree with Dr. Coe that pachysalpingitis, or thickening of the lining membrane of the tube is so rare as he represents. In his experience it is quite common.

As to dilatation of the tube with fluid, it is not so common as many suppose. He has found but eight cases altogether in which he could detect apparent swelling of the tube. In few of them he aspirated, and drew off a watery serum which contained no ciliated epithelium. He agreed with Dr. Wylie as to the propriety of being guided principally by the symptoms in determining as to the advisability of an operation in any given case.

As regards the removal of normal ovaries, he did not believe in the procedure, as a rule; although he had himself performed the operation twice. One of these patients afterward committed suicide; but the other, who had previously been bedridden for years, was practically a well woman to-day.

Most of those who are acquainted with his views, know that he was very conservative in regard to these operations. So long as we in America cannot open the abdomen without exposing the patient to the danger of death, laparotomy should not be performed when the pathological condition present is doubtful; and he thought the lines should be more closely drawn than has hitherto been the case. Dr. Mundé said, in conclusion, that he did not agree with Dr. Polk in his views in regard to pelvic cellulitis and abscess. These, he thought, actually existed in the pelvic cellular tissue.

Dr. E. NOEGGERATH said that he was glad to hear the views expressed by Dr. Polk. They are practically the same that he had himself expressed in 1872, viz., that the large majority of cases of peritoneal inflammation are of intrauterine origin, and that while cases of cellulitis and parametritis do occur, it is only when there has been a lesion of the uterus, when the lymph-sac of the lining membrane of the uterus has been broken, as the result of parturition, abortion, etc. Otherwise (if there has been no lesion), the inflammation always extends by means of the tubes.

As regards the question of frequency, he said he could only report what he said in 1872. Endometritis and periovaritis or salpingitis are the most common affections the gynecologist has to treat. After childbirth, cellulitis is more apt to occur.

In regard to the treatment, he said that he was entirely convinced that it is possible to relieve a great many of the cases which are now operated on by care-

ful treatment, consisting of local and general medications.

Dr. COE, in closing the discussion, remarked that the views expressed in his paper were based on anatomical studies, and he was, consequently, disposed to regard with some doubt a diagnosis resting solely on clinical observation. Feeling that the position which these anatomical studies had caused him to take, might perhaps be more radical than that held by some other pathologists, he had written to the distinguished microscopist, Dr. William H. Welch, now of the Johns Hopkins University, Baltimore, who has had a very large experience in the class of cases in question, and Dr. Welch had written to say that he believed that this position was the correct one. He stated that he saw very few cases of pyosalpinx, and that he was continually receiving ovaries and tubes, removed by surgeons, which were, so far as could be made out, entirely free from disease. He said, in addition, that it was in his opinion a mistake to suppose that the so-called cystic ovaries so frequently extirpated would, if allowed to remain, degenerate into true cystoma.

CINCINNATI ACADEMY OF MEDICINE.

Stated Meeting, March 29, 1886.

THE PRESIDENT, JAMES T. WHITTAKER, M.D.,
IN THE CHAIR.

THE TREATMENT OF CHRONIC CYSTITIS.

DR. P. S. CONNER, in opening the discussion of this subject, said that in a state of health the bladder admirably performs its physiological function, simply that of holding the gradually formed urine until such time as micturition may be convenient and proper. Neither in the viscus itself, its contained fluid, nor in the canal through which it is discharged, is there a source of irritation. But let there be a long-continued altered state of the inflowing fluid, a tumor of the bladder wall, a retained foreign body in the cavity, or an obstacle to the ready outflow of the urine through the urethra, and sooner or later, in greater or less degree, there will be developed a state of chronic inflammation, with associated chemical changes in the constitution of the fluid. In any case, therefore, the diagnosis being established, before any method of treatment is instituted, determination must, if possible, be made of the exciting cause, the removal of which, if ascertained and capable of being taken away, should be effected at the earliest moment. Attention was then called to the proper adaptation of treatment to the location of the cause, whether in the kidney, the bladder; or the urethra. Because of the ready removal of the condition which produces the cystitis associated with stone or stricture, such cases of chronic inflammation of the bladder, if they have not existed so long as to have become complicated with lesions of the kidney, can be cured with comparative ease. It stands to reason that if the inflammation is excited, maintained, or aggravated by the irritant character of the urine, just in proportion as such is lessened, will the morbid state be diminished, and this can be effected in no slight degree by increasing the amount passing through the bladder. To such flushing of the cavity, are due the favorable effects of the administration of large quantities of water, either any ordinary soft

water, or that obtained in a state of purity from certain springs. Some of the infusions owe a part at least of their reputation to the good effect of the cleansing of the bladder by the increased amount of urine passing through. For like reasons, washing out the bladder by injection acts very beneficially, provided it be properly done and with a suitable fluid. The instruments must be clean, in the fullest surgical sense of the word, the fluid warm, and such as to arrest and prevent decomposition. Theoretically the weak sublimate solution is the best that can be employed, but practically it is found not infrequently to be badly borne, even when of a strength of not more than 1 part to 6000, 7000, or 8000. Care must always be taken not to throw in the fluid too rapidly or too strongly, and, if a single-current catheter be used, not to overdistend the bladder.

In elderly men an existing cystitis, if neither stone nor stricture is present, almost always depends upon enlargement of the prostate, in a small proportion of cases confined to the third lobe. Such enlargement acts as a stricture does, producing the same condition of the urine and of the bladder wall. In the earlier stages and the milder degree, the indications for treatment are very clear. Systematically and at regular intervals, the bladder must be completely emptied, and that with the least possible effort to the patient; in other words, catheterization should be made and natural evacuation altogether suspended; suspended, not abandoned, for not infrequently, after the steady use of the catheter for many months or years, patients may, without resulting ill effects, permanently lay aside the instrument. As a rule, however, the subject of prostatic hypertrophy in amount sufficient to produce notable effects upon the bladder and urine, should make up his mind to employ the catheter uniformly during the rest of his lifetime, the soft instrument preferably, and always properly disinfected. The chemical changes in the urine, the speaker said, are in many cases due to the introduction of germs on the catheter that is used, so that the frequent use of the instrument has long been recognized as likely to be, if not certain to be, followed by putrefactive changes in the urine and an aggravation of the symptoms. In the older and severer cases, where the general hypertrophy of the prostate is great, the basal fund of the bladder consequently deep, the residual urine in decided amount, and alkaline in reaction, catheterization and antiseptic irrigation are the essentials of treatment. This, together with the regulation of the general habits of life, will ordinarily keep the patient in a state of comfort. Even when the muscular coat of the bladder has become so enfeebled as to permit overdistention, this method will frequently produce marked improvement in the general and local state. But oftentimes the bladder, instead of being overtolerant is excessively intolerant of fluid, the presence of a very moderate quantity of which is sufficient to excite a spasmodic, painful, imperious desire to urinate. This condition may be associated with and dependent upon prostatic hypertrophy (very apt to be of the ball valve variety, or complicated with urethral stricture), upon bladder tumor, or upon tubercular disease of the prostate or the seminal vesicles.

The intensity of the irritability of the bladder is many times markedly affected by the habits and mental state of the individual. In these cases the plan of treatment

already indicated may be sufficient so greatly to lessen the severity of the symptoms as to render the patient unwilling to have anything else done.

With reference to the permanent retention of the catheter in those cases in which its introduction is very difficult, the speaker said that, in his judgment, such a plan is not good, for, even if a soft catheter is employed, it will soon become encrusted, it will generally increase the irritation, and it will be very difficult, if not impossible, to keep it aseptic.

In many cases no soft instrument can be introduced. It is in these cases of severe character, and often of irritable nature, that operative interference is so strongly called for and is capable of accomplishing so much. Rest and the opening of the contracted urethra are the measures recommended. If there is prostatic enlargement, he continued, what can be done? Only a few years ago, it was hoped that in parenchymatous injections of the enlarged gland we should find a means of producing shrinkage, but extended experience has demonstrated that it is utterly valueless. Prostatotomy or prostatectomy, either internal or external, have much to commend them. In all the severe forms of chronic cystitis in the male, it was the opinion of the essayist, that either perineal section or suprapubic cystotomy should be made. By a comparatively slight operation—the opening of the membranous urethra—we may readily get at the prostate, recognize the position of and so be able to remove by knife, punch, or snare, a bar or ball obstructing the neck, and, in the majority of instances, be able with the finger to sweep a part or the whole of the mucous wall of the bladder, thus locating any tumor that may be present. Through such perineal opening, any existing prostatic or vesical calculi may be removed. Further, and more important, thorough and complete drainage of the bladder may be secured, a full-sized tube being easily introduced, comfortably borne, and easily removed, no such objections lying against its use as lie against a permanently retained catheter passed through the unopened urethra. Experience has shown that the presence of a drainage tube thus employed causes a dilatation of the neck and prostatic portion of the canal that may be expected to be permanent, and which will permit, after some weeks or months, of the removal of the tube and the allowing of the healing of the wound. By draining off the urine as fast as it comes into the bladder, there is secured to the organ that rest which, above all things else, is the essential element in the treatment of any surgical affection. If the cystitis depends, as it probably does much oftener than is commonly supposed, upon a vesical tumor, the perineal operation permits of the determination of the location, size, and nature of the neoplasm, of its removal, if practicable, and, under all circumstance, of the cleansing and draining of the cavity.

In those distressing cases, met with usually in young subjects, of cystitis dependent upon local tuberculous deposits, where the symptoms of stone are so closely simulated, perineal section with dilatation or division of the gland gives relief, and that more speedily than anything else that can be done. Guyon and his followers of the French school, urge that the opening into the bladder should be suprapubic, and there are unquestionable advantages in such an operation over the perineal one, with, however, associated disadvantages, so

that extended experience alone will suffice to indicate clearly which should be regarded as preferable in the ordinary run of cases. In any and all forms and grades of chronic cystitis, the prime indications of treatment are to remove the cause and give the organ rest, and just in proportion as these indications can be fulfilled will relief be afforded and a cure effected.

DR. J. H. TATE remarked that, owing to the importance of the treatment of this disease, especially in the chronic form, physicians have for some time watched with interest to see what their *confrères* would devise for their relief. The experience of the speaker had been confined rather exclusively to the form of the disease which occurs in women, particularly that coming on after parturition. Reference was then made to the obscure etiology of these cases; but many of them, he believed, were due to the use of instruments in parturition or to long-continued pressure of the head. With regard to their treatment, he had frequently found dilatation of the urethra to be followed by speedy cure.

DR. JOSEPH RANSOHOFF wished, however, to lay stress upon one point, namely, that inflammation of the bladder is rarely, if ever, a primary affection, that it is only one of a complex of symptoms, and that we are in all cases to search for the cause. In acute cystitis, the cause is usually a gonorrhœa; in chronic cystitis, it is, nineteen times in every twenty, a stricture of the urethra, or a hypertrophy of the prostate. The speaker did not believe that in a cystitis that is chronic anything can be done for its permanent relief, except through the removal of the cause. That form which depends upon hypertrophy of the prostate and on which the essayist has laid so much stress, is the most difficult form to treat. We are often called upon to treat cases of enlarged prostate in which the first symptom to direct attention to the true condition is the retention of urine. What shall we do in a case of this kind? If we can get into the interior of the bladder once, it is exceedingly likely that we can get into the interior of the bladder again; hence, if we can withdraw the urine by this means, it is preferable to do so without allowing the catheter to remain. If we cannot get into the interior of the bladder we must resort to puncture of the viscus through the anterior abdominal wall or the rectum with a trocar or an aspirator; the trocar puncture has been now almost totally discarded, however, and justly so. But death has resulted also from the use of the aspirator. With regard to allowing the catheter to remain in the bladder for a week or ten days, the speaker said that it is quite a common practice, for when a man has with great difficulty gotten into the bladder, it is with no little hesitation that he withdraws the instrument. It is, however, he added, as bad a method of practice as can be adopted. It is impossible to allow a catheter to remain in the bladder in that way and have it remain aseptic. A decomposition, as it has repeatedly been shown, can take place in the urine only upon the introduction of germs. A simple retention of urine cannot produce an ammoniacal decomposition. Hence the risks are rendered much greater by permitting this source of infection to remain. If we cannot get the catheter in, we should aspirate repeatedly if necessary, until the cause of the retention disappears spontaneously or can be relieved by an operation. Attention has been called to the supra-

pubic operation for affording a permanent exit for the urine. It was Sir Henry Thompson who first called attention to this mode of operation, but the speaker expressed doubt as to whether the method has been much followed. None of those operated on has lived long, and where there is concentric hypertrophy of the bladder and an instrument cannot be introduced the suprapubic operation is far from easy. We have then only perineal section left. This is an operation which *per se* is comparatively void of danger. It is an operation which is very easy to perform. Of course, the operation is often to be performed in persons advanced in years as well as advanced in disease, whose kidneys are often extensively involved, and in whom any operation will be dangerous, but notwithstanding this fact this operation should always be given the preference to allowing the catheter to remain in the urethra for any length of time. The operation of perineal section is one of the simplest in surgery, when an instrument can be introduced, and one which any physician should be able to perform in an emergency without occasioning the delay of waiting for the arrival of a specialist. It does not require an extensive knowledge or experience in surgery. The operation should be performed early, and if performed early it gives the operator the opportunity of getting directly at the hypertrophied prostate. The first attempts at removal of a part of the prostate were very crude, simply cutting, tearing, crushing the gland with the blades of an instrument ten to twelve inches removed from the operator's hands; so it is not remarkable that these operations were abandoned. But with the opening in the membranous portion of the urethra we can, in most cases, get the entire prostatic portion of the urethra under the finger, and it is easy to get into the gland or into the prostatic portion of the urethra, and with a sharp spoon scrape away the protruding portion of the gland, or remove it with a snare or a galvano-caustic loop. This operation has been quite often performed by Bottini even without preliminary perineal section.

The speaker then referred to several cases in which he had performed this operation, and remarked in conclusion that he had yet to see a case in which he regretted having performed it.

In reference to the cause of certain cases of cystitis as well as their treatment, he said that he could not understand why we may not have the same or a similar condition of affairs about the neck of the bladder, where an exceedingly rich venous plexus is found, that we know to exist at the extremity of the rectum. We have there, with difficult defecation, a hemorrhage which is due to nothing but a varicose dilatation of the veins there situated. We have a plexus of veins about the neck of the bladder; I do not see, therefore, he said, why we may not have just such a "varicose" condition of the veins about the neck of the bladder. A case which he considered of this nature recently came under his observation. The individual had suffered from severe hemorrhage of the bladder. No stone could be detected; in fact, nothing was apparent. He made perineal section, performed divulsion of the neck of the bladder and the individual recovered promptly.

DR. THADDEUS A. REAMY remarked that he was very much surprised to hear the statement of his friend, Dr. Ransohoff, that ammoniacal decomposition of the

urine cannot occur in the bladder without the introduction of germs by means of instruments. This demonstration has not been made, and if it has been made, its fallacy should at once be refuted. It is against all clinical experience. No subjects are so prone to suffer the consequences of ammoniacal decomposition of the urine as old women, who have more or less pronounced cystocele. Urine being retained in the vesical pouch undergoes decomposition, and these patients are great sufferers from cystitis.

That germs promote decomposition of urine in these cases will not be denied, but they are not introduced by instruments. As his time was limited, he confined his remarks to the treatment of cystitis in women past the middle of life. Most of the cases are associated with cystocele. This occurs most frequently, of course, in women who have borne children. In most of these cases it will be found that the cystocele comes on after certain changes which occur in the vagina and pelvic tissues: the absorption of fat, atrophy of muscular fibre, and a change in the conformation of the bladder. The senile atrophy of the uterus has removed the infravaginal portion of the cervix, and, therefore, obliteration of the vaginal culs-de-sac. Under these circumstances, as already stated, cystocele occurs, and with it chronic cystitis, which cannot be cured so long as the cystocele is unrelieved.

There are two methods of relief, one palliative, the other radical. Palliation answers in many cases and should always be adopted when it can be successfully done. It consists, of course, in support of the prolapsed portion of the bladder. For this purpose, Skene's pessary answers in some cases admirably, in others he had found far better results from the use of the common watch-spring, rubber-covered, ring pessary. He referred to the well-known modification of Meigs's pessary. This instrument is easy of introduction, and its elasticity prevents it from causing pain or irritation, provided the proper size is selected. As to size, great care is necessary. One of the chief advantages of the instrument is the certainty of its retention in the vagina. Owing to the vaginal changes to which he had already referred, he had found no other form of pessary that is not prone to drop out—a very great inconvenience.

He had ordered made, but has not yet tried, a modification of this instrument which he felt confident would prove of great advantage. This modification consists in attaching two of these ring pessaries, one smaller than the other, so that they are separated about a third of an inch. On introduction the smaller ring is placed toward the uterus. Thus there are two joists instead of one for support of the prolapsed pouch. Should occasion require, three rings could be employed.

The radical method of cure is the removal of a portion of the anterior vaginal wall. The speaker had in several instances removed in this operation a portion of the bladder wall. If care is taken to close the wound with silver sutures, as in the ordinary operation for vesico-vaginal fistula, there need be no fear of evil consequences from this radical measure. The great attenuation of the vaginal wall renders this operation quite easy of performance, and fortunately in cases where the vaginal wall is not so thin, there will usually be found no necessity for carrying the operation so far as to include bladder tissue.

He not unfrequently encountered cases of cystitis in women, of such long standing, with such changes in bladder structure as to render it futile to waste time on any other method than bladder rest, with better opportunity for topical application to the diseased mucous membrane. He does not hesitate in these cases to make a vesico-vagina fistula, making the opening large enough to insure free drainage and at the same time allowing of greater facility in medicating the cavity. He had allowed extreme cases to remain open as long as seven months. He had seen cases of the most desperate character thus cured.

He stated that it is his invariable rule in the above cases to make the opening large enough to admit of thorough exploration of the bladder by the finger for discovering and removing any neoplasm or foreign body that may be present.

He had found dilatation of the urethra seldom useful for relief of vesical irritation, except in nervous or hysterical patients. These are the subjects in which irritable spasm of the urethra occurs. In such subjects he had seen dilatation act as by magic. He uses for the purpose the graduated metallic or hard-rubber dilator, never dividing instruments. The operation is generally done too rapidly. At least an hour should be occupied at a sitting. By such caution more permanent good is obtained; and, though the dilatation be carried to an extreme degree, permanent incontinence will seldom follow. He had never had this misfortune occur in his own practice.

He would not enter upon the medical treatment of cystitis in cases where abnormal urine plays the greatest rôle, since this does not differ materially in the female subject from what is needed in the male. He must, however, corroborate what had been said as to the value of the *tr. ferri mur.* for correction in dyspeptic subjects, and often in pregnant women when the abnormality consists in the amorphous phosphates of lime, and ammonia-magnesian phosphates. Under twenty-five drop doses of this agent four times daily in such cases the quantity of urine is increased, probably as a result of increased blood pressure, and the reaction of the urine is within a few hours normal, vesical irritation at once subsiding.

DR. DANIEL YOUNG remarked that he had made it a rule of practice always to rely upon the milder methods of treating cystitis before resorting to those of a more severe character. Dilate the stricture, if one be present, put the patient at rest, and place a suppository containing morphine in the rectum. In most of the cases which had come under his observation, both in hospital and private practice, this was all that was required for their relief. He had further observed more mischief done by the practice of washing out the bladder than the good that can be claimed for it. He had been particularly impressed with the increase in the number of cases he was called upon to treat at the hospital in the five years which immediately succeeded the introduction of this method of treatment. More severe cases of cystitis were admitted during those five years than in any preceding five years, or during any period of the same length that has followed, and in most of them there was a history of irrigation previous to their admission to the hospital. In regard to the quantity of fluid which should be used in washing out the bladder, it has

been stated that it should be small. There is a great deal of difference in the estimate that is placed upon a small quantity. In reality, no rule can be laid down for the measurement of this quantity, for every case is a law to itself. In some cases a very small amount will cause irritation, while in others several ounces will be tolerated with impunity. The speaker doubted the propriety of operative procedures in cases of long standing, for in a majority of such cases the kidneys will be found to have suffered greatly, and to have undergone such advanced destructive changes as to render valueless any operative measures that might be undertaken.

DR. CONNER, in closing the discussion, remarked that there was much of truth in the statements of the last speaker regarding irrigation of the bladder. It would, however, be remembered that in his paper he stated that the bladder must be properly washed out, by a proper method, with a proper material; for if it be not so done, it will accomplish no good but only harm. The more he sees these cases, the speaker remarked, the more he was convinced that the operation, though simple in statement, is not simple in fact. He did not regard perineal section as one of the simplest in surgery, and one that requires little or no skill and experience. The more he had occasion to make the operation, the more he was impressed with the fact that the exploration of the bladder through a perineal opening might be made a difficult or even an impossible thing at the time of operation. Not long ago he had a case of stone in a man of good physical development, in whom it was impossible to explore the bladder with the finger, although he had made the lateral operation. He was further convinced that in the digital exploration of the bladder more is to be learned than one thinks at first. It is possible, in this way, to detect the existence of a ball valve enlargement of the third lobe, which is not so very infrequently the cause of the difficulty.

It is not impossible, by any means, the speaker continued, to drain the bladder by the suprapubic opening. If the tubes be properly inserted, the urine can be readily drawn off. If the bladder be in a state of moderate distention and properly elevated, it is not a matter of difficulty to find it. In the recent reports from the French hospitals, and particularly from the service of M. Guyon, much information has been furnished respecting operations made in this way.

In conclusion, the speaker remarked that he could readily understand how the use of the pessary referred to by one of the speakers would be followed by relief in cystitis of the female bladder; for the pouch that has been said to exist in the bladder is identical in the effects it produces with that which is formed in the male bladder by a hypertrophied prostate, and by the pessary the lower wall of the bladder is so lifted up that the retaining pouch no longer exists.

Stated Meeting, April 5, 1886.

DR. P. S. CONNER presented a specimen which he had secured since the last meeting of the Academy. He said that the day following the discussion of the treatment of chronic cystitis, in which he had referred to the occasional presence of a ball valve obstruction in the urethra, he had been called upon to operate in a case of this affection. After making a perineal section, he could distinctly feel a prominence in the region of

the third lobe of the prostate. This was, without much difficulty, detached, when it was found to be in reality double. Each portion was, when presented, of a conical form, smooth and quite firm; at the time of operation they were very hard. Either one of them would serve completely, or very nearly so, to close the mouth of the urethra. The speaker remarked that he had in his possession, also, specimens which showed very distinctly the bar and ball valve arrangements. The patient in this case was suffering, also, from a large abscess of the abdominal wall, which ruptured internally, and as he was too much reduced to undergo a laparotomy, he died within a few days after the operation.

DR. WILLIAM JUDKINS reported favorable results in a few cases from the administration of *herniaria glabra*. The cases were, however, of rather an acute character. The specimens of the drug—the dry herb and the fluid extract—had been presented to him by Prof. J. U. Lloyd, of this city, with a very complete history of the plant, its preparations and uses, which the speaker read in full. As its name imports, it was used in early times in the cure of hernia. It then came to be considered a diuretic, but finally fell almost entirely out of use, and is now little known except in the vicinity of Cincinnati, where there is still considerable demand for it. In conclusion, the speaker remarked that while he had seen the most marked results from its use in acute cases, he had found it of great service in others of a chronic form.

DR. JAMES T. WHITTAKER remarked that he had met with what he thought was a rather unusual number of cases of chronic cystitis, most of them grave, in association with stricture of the urethra in young men, and with enlargement of the prostate in the old. The diagnosis of a catarrh of the bladder is frequently indiscriminately made. He frequently had patients come to him with this diagnosis, made either by physicians or by the patient himself, in which no condition warranting the application of the term was present. Many of these cases are examples of what is known as phosphaturia, and a very small minority of them are cases of spermatorrhœa. Phosphaturia is not so rare, the speaker thought, as is generally stated. It has nothing to do with catarrh of the bladder, and in no manner resembles it, except in the fact that deposits are found in the urine. Phosphaturia is simply a neurosis of the kidney. The phosphates are secreted in this affection in such abundance that it is not uncommon to find a patient presenting a specimen in which there is a precipitation of phosphates a quarter or half an inch in depth at the bottom of the vessel. The treatment is very simple, the difficulty being usually quickly cleared up by the administration of a few drops of phosphoric acid several times a day.

With reference to the origin of chronic cystitis, the speaker agreed with the statement made by one of the speakers at the last meeting, that it is a secondary disease. In his experience he had never seen a cystitis that could be attributed to cold, and if such cases occurred they must be exceedingly rare. The cases he had met had occurred, as stated, incidental to gonorrhœa, for the most part in young men, or as a result of enlargement of the prostate, after fifty-two, which is, according to Sir Henry Thompson, the proximal limit

of the disease. Among the cases resulting from gonorrhoea he included those which were due to stricture of the urethra.

The diagnosis of cystitis is generally easy; there is pain associated with tenderness on pressure, and tenesmus often so great—always, in fact, in a bad case—that a little blood is extruded with the urine. This symptom makes itself most manifest at the close of urination, when the sphincter contracts into a sudden spasm. He had never seen it more marked than in a case which he had treated with Dr. Conner, where the individual, on account of refusing an operation, had to be kept under narcotic doses of opium for nearly a month. There is, further, an easy way of recognizing the grade of catarrh present in the bladder; that is, by simply testing the urine. In the milder cases, the urine is still acid, but in the graver cases it will be found to have become alkaline. These patients suffer from the disease paroxysmally. They do not always present the dire picture which was portrayed by a former speaker. They attribute the acute exacerbation to cold. They have been getting along comfortably for months, drawing their urine with catheters, when suddenly there is an acute exacerbation which brings the patient to his physician. If he can tide the patient over for a few weeks he will bring him back to his former chronic state, in which he may live for years in comparative comfort. We cannot often persuade a patient, more especially an old man, to undergo an operation; there is, in fact, nothing harder than to persuade an old man to have an operation performed on an enlarged prostate which he has endured for years. It may be, too, that he recalls cases in which an operation resulted fatally. The young man also will often hesitate a long time before he will consent to have his perineum split open. Young men will generally acknowledge that their disease is the result of an old gonorrhoea, of which they have never been entirely well; that is, there has been at times a reminder in the way of burning, or slight strangury, which finally culminates in the attack. We will also find in these cases that there is more or less constant discharge of thin shreds of tough mucus, so characteristic of gonorrhoeal inflammation—"Tripperschleim," the Germans call it.

With reference to the irrigation of the bladder, the speaker remarked that it must be practised with the utmost care, or it would bring the physician under criticism like that of a speaker at a previous meeting, who said that he did not believe in it at all. A man who distends the bladder in a state of acute inflammation will do his patient the greatest harm. The bladder should never be washed out with a fountain syringe or with a hard-rubber syringe. It should be done with a very delicate soft-rubber syringe, which the surgeon holds in his own hand, regulating the pressure very carefully. This care should be taken also as to the catheter used. Never use anything but the soft catheter. Under no circumstance should a double catheter be used. With it the bladder is washed out under full distention. The individual having his bladder washed out should never be on his back; he should be in the sitting or standing posture, in the position in which particles of mucus or pus are most easily floated out. Only in the most severe cases should the patient be allowed to remain on his back.

It is a good plan to limit the amount of water the patient drinks. It is well to direct him to drink just as little as possible to assuage his thirst. The greatest damage is often done by allowing these cases to go to watering places; the speaker had lost two cases from this cause, the patients having been persuaded by their friends to try such resorts. The point is to eliminate the water in other ways.

There is only one medicinal treatment, and that is opium in sufficient doses to subdue the cramplike contractions. The best preparation of opium for this purpose is the Dover's powder, which may be made a pleasant medicine by giving it in the form of a syrup. A teaspoonful of this syrup allays the pain better than anything that can be given. The advantage of it is that, with the action of the opium in allaying the pain, we get a sharp diaphoresis—the patient should, of course, be in bed. After awhile there comes the necessity for the administration of stronger preparations of opium. Sometimes great benefit comes from injecting the tincture of opium into the bladder. The mucous membrane in health has little absorptive power, but where the epithelial coat of the bladder is destroyed it is quite different, and the bladder suffering erosion or ulceration will absorb medicines very readily. Some physicians prefer the use of suppositories. No objection can be made to this method.

These are very little points, the speaker said, but they had proved very valuable points to him. Of course, he added, in the presence of mechanical obstacles like those exhibited this evening, there is nothing to be done but surgical interference. In these cases the physician is frequently struck by the apparently anomalous condition that he meets. The catheter will pass readily into the bladder, but on its removal the urethra is completely occluded. It is not right to say that there is no relief for these cases, for there is no relief to be compared to that which is afforded the patient who undergoes a proper operation.

There are no remedies older in medicine, more tried or more relied upon than the turpentine, but it is in the mild cases and in the acute cases that these remedies are of most value; and a remedy of more value than any of these in acute cystitis is the salicylate of soda. All these remedies are valueless in the chronic cases with which we have to do to-night. None of them is to be compared in its effects to the hot sitz bath. The speaker further objected to the statement that these cases are produced by acid. This cause of a disease is very difficult to establish. Again, he asked, how many of the members of the Academy have seen the disease produced by cantharides? We may have seen it when we were students, when that remedy was more frequently applied, but now it is exceedingly rare.

As has been already stated, the vast majority of cases of chronic cystitis are those which are caused by obstruction. They are the cases in which we observe those striking nervous phenomena—beautiful in a pathological sense—cases with shooting pains down the thighs, psychical disturbances, and melancholia. The ancients knew them as *hysteria virilis*, *hysteria masculina*, and vindicated the use of the term in the fact that the prostate gland is the analogue of the uterus.

NEWS ITEMS.

A PASTEUR INSTITUTE.—A commission appointed by the Academy of Sciences has decided upon the establishment of a *locale* in Paris for the treatment of persons bitten by mad dogs. It is to be called "Institut Pasteur," and is to be supported by public subscription. As it is intended to be international, subscriptions are solicited from all parts of the world. A cable despatch announces that already about \$10,000 have been raised.

JEFFERSON MEDICAL COLLEGE.—Prof. William H. Pancoast has presented his resignation from the Chair of Anatomy in this school. We understand that the following gentlemen have been nominated for the position thus left vacant: Dr. W. W. Keen, Dr. W. S. Forbes, Dr. J. E. Mears, Dr. O. H. Allis, and Dr. Geo. McClellan. The election will take place this week.

THE LAW OF MEDICAL REGISTRATION.—The law requiring physicians to register with the county authorities has recently received a rather rigid interpretation from Judge McPherson, of Lebanon County. Dr. Ege, a properly registered physician of Berks County, was summoned to attend a patient across the line in Lebanon County, where he was arrested for violating the Registry law. The case came before Judge McPherson, who, after holding it under advisement, finally fined the offending physician \$100 and \$50 costs. Dr. Ege's counsel took the apparently tenable ground that the fact of his client's registration in Berks County practically fulfilled the requirements of the act, and that the law did not contemplate that a physician should register in every county he might visit professionally; but the Court held to the contrary. The case will be carried up to the Supreme Court, and its outcome from that tribunal will be awaited by the medical fraternity with a good deal of interest.

SANITARY CONVENTION IN PHILADELPHIA.—A sanitary convention, the object of which will be to afford an opportunity for an expression of opinion on matters relating to the public health and the discussion of methods looking toward an advancement in the sanitary condition of the Commonwealth, the prevention of sickness and avoidable death, and the improvement of the conditions of living, will be held in Philadelphia, under the auspices of the State Board of Health, on Wednesday, Thursday, and Friday, May 12, 13, and 14, 1886.

TENNESSEE STATE MEDICAL SOCIETY.—At the annual meeting of the Tennessee State Medical Society, held at Memphis on April 6th and 7th, the following officers were elected for the ensuing year:

President.—Dr. W. T. Briggs, of Nashville.

Vice-Presidents.—Drs. J. W. Penn, of Humboldt, and J. B. W. Martin, of Nashville.

Secretary.—Dr. Ambrose Morrison, of Nashville.

Treasurer.—Dr. Richard Cheatham, of Nashville.

The Society adjourned to meet in Nashville on the second Tuesday in April, 1887.

OBITUARY RECORD.—At Middletown, Conn., on April 12th, ABRAM MARVIN SHEW, M.D., Superintendent of

the Connecticut Hospital for the Insane. On February 20th he accidentally fell down the main stairway of the hospital and injured his spine. He had been confined to his room for several weeks, but was apparently getting better when a stroke of apoplexy rendered him unconscious for several hours, and resulted in his death. He was born in Watertown, Conn., in 1841. He served two years in the United States Army as surgeon, and for three years he was attached to the New Jersey State Hospital. Dr. Shew superintended the building of the Middletown Hospital, and since its opening, in 1868, he has been at the head of the institution.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT OF THE U. S. ARMY, FROM APRIL 6 TO APRIL 12, 1886.

HARTSUFF, ALBERT, *Major and Surgeon.*—Granted leave of absence for fifteen days.—*S. O. 71, Dept. East*, April 7, 1886.

LAUDERDALE, JOHN V., *Captain and Assistant Surgeon.*—Ordered from Dept. Dakota to Dept. Texas.—*S. O. 79, A. G. O.*, April 5, 1886.

ADAIR, GEORGE W., *Captain and Assistant Surgeon.*—Ordered from Dept. Dakota to Dept. East.—*S. O. 79, A. G. O.*, April 5, 1886.

FINLEY, JAMES A., *Captain and Assistant Surgeon.*—Ordered from Dept. Texas to Dept. Dakota.—*S. O. 79, A. G. O.*, April 5, 1886.

KILBOURNE, H. S., *Captain and Assistant Surgeon.*—Ordered from Dept. Dakota to Dept. Columbia.—*S. O. 79, A. G. O.*, April 5, 1886.

GARDNER, E. F., *Captain and Assistant Surgeon.*—Ordered from Dept. Columbia to Dept. East.—*S. O. 79, A. G. O.*, April 5, 1886.

GRAY, WM. W., *Captain and Assistant Surgeon.*—Ordered from Dept. East to Dept. Dakota.—*S. O. 79, A. G. O.*, April 5, 1886.

BANISTER, J. M., *Captain and Assistant Surgeon.*—Ordered from Dept. East (upon the expiration of his present leave of absence) to Dept. Columbia.—*S. O. 79, A. G. O.*, April 5, 1886.

CARTER, E. C., *First Lieutenant and Assistant Surgeon.*—Ordered from Dept. Arizona to Columbus Barracks, Ohio.—*S. O. 79, A. G. O.*, April 5, 1886.

JOHNSON, R. W., *First Lieutenant and Assistant Surgeon.*—Ordered from Dept. Dakota to Dept. East.—*S. O. 79, A. G. O.*, April 5, 1886.

WILSON, GEORGE F., *First Lieutenant and Assistant Surgeon.*—Ordered from Dept. Columbia to Dept. Dakota.—*S. O. 79, A. G. O.*, April 5, 1886.

MERVILL, JAMES C., *Captain and Assistant Surgeon.*—Granted leave of absence for three months.—*S. O. 81, A. G. O.*, April 7, 1886.

BIART, VICTOR, *Captain and Assistant Surgeon.*—Sick leave of absence still further extended one year, on account of sickness.—*S. O. 79, A. G. O.*, April 5, 1886.

BIRMINGHAM, H. P., *Captain and Assistant Surgeon.*—(Camp Grant, New York City) temporarily assigned to duty at Fort Columbus, New York Harbor.—*S. O. 72, Department East*, April 8, 1886.

ROBERTSON, R. L., *Assistant Surgeon.*—On expiration of his present leave of absence, will be relieved from duty in Dept. Texas and will report in person to Commanding General Dept. Dakota, for assignment to duty.—*S. O. 78, Department Dakota*, April 3, 1886.

OFFICIAL LIST OF CHANGES IN THE MEDICAL CORPS OF THE NAVY, FOR THE WEEK ENDING APRIL 10, 1886.

DRAKE, N. H., *Passed Assistant Surgeon.*—Detached from duty at Naval Hospital, Philadelphia, Penna., and ordered to duty at Naval Hospital, Brooklyn, N. Y.

FITTS, H. B., *Passed Assistant Surgeon.*—Detached from duty at Naval Hospital, Brooklyn, N. Y., and ordered to duty at Naval Hospital, Philadelphia, Penna.

ANDERSON, FRANK, *Passed Assistant Surgeon.*—Detached from Naval Laboratory, N. Y., and granted six months' leave, from May 1, 1886, with the privilege of going abroad.